ICE OR WATER

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ANOTHER APPEAL TO INDUCTION FROM THE SCHOLASTIC METHODS OF MODERN GEOLOGY

BY

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IN THREE VOLUMES

VOLUME I.
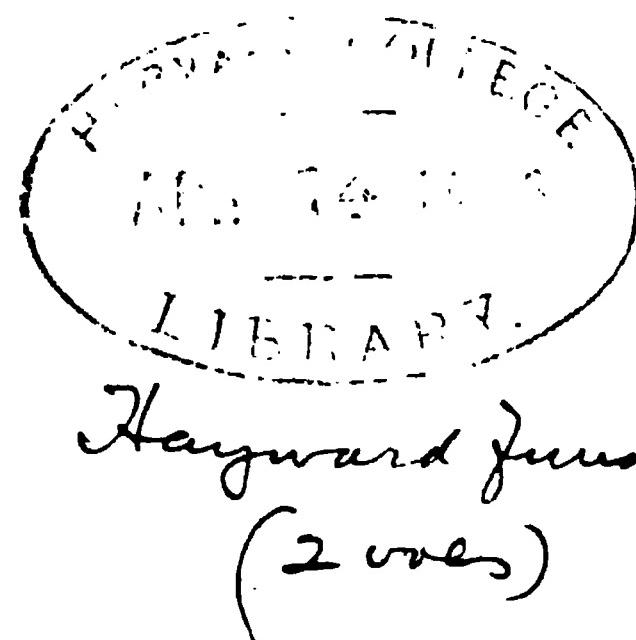
I DEDICATE this book, which has cost me some years of work and thought, to an old friend with whom I have spent some happy hours.

He has excelled as a philosopher, as a politician, as a master of clear and graceful English, and last, but not least, as a most acute and accomplished debater and critic. He has maintained at its highest level the traditional rectitude and courtesy of the great leaders of English public life. He has shown how it is possible amidst the engrossments of practical politics, which dominate even the leisure of a Prime Minister, to keep up an ardent interest in science, and has helped to define those more difficult philosophical problems which baffle all science.

I had the privilege of hearing him read and of sharing in the discussion of his first book before it was published, in his beautiful home in Haddingtonshire, and the still greater privilege of having had his criticisms upon some of the polemical issues which have greatly occupied me, and notably upon those involved in the following work, with the general conclusions of which I believe him to be largely in sympathy.

He will not, I hope, resent my wish to associate these volumes with his name. They lack unfortunately the tasteful diction and methodical arrangement in which he has enshrined his own speculations, but I believe them to contain an honest and laborious analysis of one of the most difficult and important problems dealing with the earth's past history, a problem which is intimately connected with

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the difficulties surrounding the first appearance of man and the earliest chapters of human history.

This problem greatly exercised the distinguished founders and creators of the science of geology. Their solution of it has been latterly displaced by the teaching of another school which has substituted scholasticism for induction and unbounded speculation for reasoned hypothesis and which has been leading men away into the desert fields of scientific obscurantism. Against this movement my book is a protest, and I hope I have done something in it to justify a return to the saner methods and views of the old masters of our science, and something also to secure the sympathy of the author of *The Foundations of Belief.*

PREFACE.

GEOLOGY, which is the youngest of the sciences, is the one in which mediæval methods and mediæval logic prevail the most. This is doubtless to be partially accounted for, because it deals with problems in which the theologian claims to have a voice. That is not the only reason. For more than a hundred years there has been a continuous struggle between two rival schools of geologists, who have based their conclusions upon widely different premises. One of them, the older one, now almost extinct, constantly, and as a matter of course, appealed to causes no longer in operation, to explain by no means mysterious effects. The other, on the contrary, has deemed this method of scientific argument to be utterly vicious. It has contended that we are not justified under any circumstances in a scientific induction, in appealing to causes unknown to human experience, and that the only true scientific method is to examine with care and pains the forces at this moment in operation, and to explain the former history of the earth by them and them alone.

It seems plain to me that both these schools have been vitiated by a common malady. I am free to confess that until we have examined and sifted every known cause now in operation, we are not justified in attributing phenomena to causes which are either hypothetical or imaginary. On the other hand, I hold that it is not true science but the reverse to attribute effects to causes, supposed to be consistent with experience, but whose competence has not been verified.

Again, it is not enough to analyse the *kind and quality* of work which a cause may be capable of initiating and carrying through. We must also analyse its amount and degree. Because a man can crack a nut with his teeth, it does not follow that he can do the stupendous work of a steam-hammer, which would be equally capable of cracking nuts. Because rain can wash away the loose sand on a mountain slope, it does not follow that it can carve a

Matterhorn out of the hardest crystalline rocks, or sweep out the solid contents of the Wealden valley. Given any quantity of time, I still hold that the dynamical forces of nature are limited in their operations, and I deem the greater problems just named to be as impossible as the lesser ones are palpably possible.

True induction requires, that we shall measure the capacity of our causes not merely in kind but in degree. It is not enough that we have found a cause capable of similar effects on a small scale, when we are judging of effects on a much larger scale. Our cause must be capable of being equated with our effect.

In addition to this, I hold that an appeal to limitless time is in itself as preposterous and irrational as an appeal to limitless force is deemed to be, by certain thinkers, and I take the greatest possible exception to a statement like the following from the pen of my distinguished friend, Mr. Whitaker, who presided so recently and so efficiently over the Geological Society of London. His words are: "As astronomy has proved the existence of almost boundless space, so geology needs almost boundless time. The former science gives us our liveliest picture of infinity, and the latter our best idea of eternity. When astronomers talk without any opposition of immeasurable space, surely geologists should be allowed immeasurable time. The last Wollaston medallist has eloquently said: 'The leading idea which is present in all our researches, and which accompanies every fresh observation, the sound which to the ear of the student of Nature seems continually echoed from every part of her works, is Time! Time! Time!'" No amount of time will enable a set of human teeth to punch holes in a steel plate. Hence when we are face to face with some gigantic problem, greatly differing in degree from any similar problem at present in solution, we must not hesitate to give increased potency to our cause in order that our induction may be sound. And for this we have surely ample justification (as I have often urged before) in some of the unmistakable monuments of Nature. If we look at the placid face of the moon, where the keenest observation has failed to notice the slightest change going on at present, we find it torn and rent in a way quite unmatched by anything on the earth, and attesting a period in its history quite unlike anything at present known. If we look to our own geological record, where are we to find anything fashioned in human memory the least like the great beds of granite and gneiss or even

the tabular masses of basalt and trachyte which occur in so many latitudes, for instance in India, South America and in Auvergne, or the quite as wonderful deposits of the chalk or the slate rocks? Can we answer our boys who ask us how these beds were formed, by pointing to any process in actual operation now, which would explain them? If we turn from these instances to those more distinctly dynamical, who is prepared to say that the riven and twisted and upheaved masses of the Himalayas and the Andes, the huge faults like that of Durham, the vast cliffs and chasms of the Alps where the tertiary beds are thrown up on end, etc., etc., are comparable in extent and degree in any way with phenomena of which man has had direct cognisance, or which are within the capacity of any forces of which he has had direct knowledge?

It is not only in regard to the extent of the work done that we must frequently in geology put aside any human tape-measure and footrule and appeal to a far more significant exercise of dynamical forces than can be measured by our daily experience, namely, that marked by the enhanced rapidity and actual violence demanded by the changes involved. This is true, I maintain, not merely of degree but also of rapidity or suddenness. No appeal to gradual changes will explain the vast ruins on the face of the moon, the similar ruins that cover Iceland or Auvergne, or the burnt land of Asia Minor and lower India; the signs of violent dislocation we see in the great African rift and in other great rifts elsewhere, and the torn and tossed masses of crystalline rocks on the Alps.

Even in human memory the forces of Nature have not been working with uniform speed or intensity, but numerous cases can be quoted of paroxysmal interludes occurring irregularly and casually. The great earthquake of Lisbon, the similar earthquakes which took place in Japan a few years ago, the intermittent outbreaks in volcanic regions, such as at Etna and Vesuvius, in the Sandwich Islands and in Java. These are only examples and types of what has been occurring on a much greater scale in special localities in all human history, and they are a standing protest against the extravagant theories of uniformity which prevail with some and which were specially pressed home by Ramsay and Jukes when they inspired the geological creed of the Geological Survey. This is not uniformity as understood by the students of other and more precise sciences. It is in fact a revolt against

true uniformity, which only means that similar effects, both in kind and degree, have had in all human probability similar causes.

In my view the only true philosophical method to adopt in geology, when we have a problem to explain, is to search for a competent cause in all the highways and byways of Nature's arsenal. If we can find any cause still operating which is competent to do the work, we shall of course accept that cause as the only one justified by the evidence. If we find a cause which is capable of producing similar effects but only on a smaller scale, we may reasonably and rightly inquire further whether the smaller cause could bear the strain of much larger demands upon it when operating over a much longer period and could then produce the enhanced effects. If we are satisfied that it could, we must not take it for granted that this factor of additional time is necessarily available. That is as much a question of evidence as any other problem.

If we eventually satisfy ourselves that we cannot legitimately appeal to enhanced time, or that the intensity of the force as measured by our experience is incapable of doing the work, we ought to have no hesitation whatever in appealing to a much greater force, similar in kind maybe, but greater in intensity or greater in rapidity of operation. That is surely the true inductive method. The only proviso which is a necessary element in such an appeal, is that the force we call to our aid shall be clearly and distinctly one consistent with a most rigid adherence to the laws of physics, and whose competence would be confirmed by the mathematician and the mechanic. All other appeals I deem to be transcendental, metaphysical, and outside the limits of true science by whatever great men they are made, and I especially protest against the notion that geology can dispense with the methods and the results of mathematics, mechanics and physics in framing its hypotheses.

This, the true inductive method, I hold has been completely ignored by the geologists who, in recent years, have described the later deposits which cover the bony skeleton of the earth with their soft mantle.

Long ago, when I was a mere boy, I was corresponding with Darwin on a subject which had always interested me, namely, how to account for the carcases of mammoths and other beasts being preserved whole, in the frozen ground of Siberia; for the vast hecatombs of buried skeletons and bones of his contemporaries

in different parts of the world; and for the great gap in the evidence and the hiatus which exists between the remains of these animals, including primitive man, and the remains of the succeeding races of men with their domesticated animals.

Darwin, like many others who have looked the problem face to face, confessed to me that it remained to him the one stupendous mystery in the later geological history of the world, for which no rational explanation had been forthcoming.

I ventured, during several years when I was working at a very different subject which necessitated a good deal of study of Russian history and science, to collect all the materials I could lay my hands upon (largely from Russian sources) in regard to this most attractive puzzle, and eventually put it together into a book, *The Mammoth and the Flood*, which, whatever its theoretical views, was a great arsenal of facts. I am glad to say it has since been largely used by many students who have not entirely shared my conclusions.

Those conclusions were the result of a frank and honest sifting of every element in the problem I could think of, and of an analysis of all previous theories in regard to it I could put my hands upon. They have never been answered, and I believe them to be in the main unanswerable. They involved a return to the geological reasoning and hypotheses of an earlier date, in which a cataclysm on a widespread scale, which caused very important dislocations of the earth's surface accompanied by gigantic tidal waves, was invoked as a *vera causa* for a phenomenon out of the reach of current forces to compass. To the conclusion I elaborated and defended I completely adhere in all essential matters, and I again challenge, as I often have challenged before, the production of any hypothesis which so completely and conclusively accounts for all the facts as the one I stand upon.

The same catastrophe with the same corollary in the shape of movements of diluvial water on a great scale was, I felt, the only ultimate explanation not only of the biological difficulties just referred .to, but of the corresponding inorganic difficulties also. It formed in my view the real key to the last touches and alterations made in the configuration of a large part of the earth's surface and of the soft deposits which cover it. This was also the view of the old masters. It has been for many years displaced in favour of another view which has been supposed to be

more in unison with uniformitarian notions, namely, by the invocation of a great Ice Age. This, it was claimed, would do everything that was needed to explain the puzzling difficulty without any appeals to that dreadful postulate a catastrophe, *i.e.*, an unexpected effort of Nature on a large scale. The Ice Age is, however, an example of a catastrophe of another kind. A great Ice Age on the scale required, with ice working in the mode represented, I have always urged, is as much outside the range of uniformity as any catastrophe can possibly be. Nothing of the kind has been known in human experience, and as it seems to me nothing of the kind is possible unless we substitute for the forces of Nature as known to us, imaginative and transcendental forces of various kinds ; substitute, in fact, a tremendous geological dream for geological induction. Hence the name I have given the glacial theory, namely, the Glacial Nightmare.

A few years ago I devoted two considerable volumes to a dissection of that theory. It was allowed by my generous critics (who did not agree with some of my results) that the book was full of facts, and that its argument was ingenuous, that is to say, that nothing was screened or hidden but every difficulty was faced. In that work I tried to sift the problem as closely as I could, and to show that while both a glacial age and an appeal to a catastrophe were departures from the doctrine of uniformity as laid down by its more extravagant geological champions, the catastrophe I appealed to was in accordance with the laws of Nature, while the glacial nightmare was not; and further, that the former was competent to do the work demanded from it while the latter was not.

Although the glacial theory as now taught is a very different glacial theory to that current when Croll and Ramsay and Green were in their heyday, and many of the younger men have largely qualified its extravagance, it still maintains a bold although a much smaller front to the enemy, and I have thought it right to supplement my former work by another on a still larger scale, and containing a still larger number of facts and arguments pressing home the conclusions I have so long held, with cumulative force.

The two volumes now published contain a large part of, though not all, my supplementary arguments against the glacial theory ; a portion being still reserved for a succeeding volume which will also contain an enlarged presentation and justification of the theory

I substituted for it in my *Glacial Nightmare*, namely, the diluvial theory.

They contain in addition a parenthesis which I deemed it necessary to publish as part of my ultimate case, but which as it stands somewhat breaks the continuity of the argument. This, therefore, I would desire my critics to pass by temporarily and then to revert, and to read it apart from the rest of the story. This parenthesis deals with the transcendental notions about denudation now current, against which I have also continuously rebelled, and is contained in chapters v., vi. and vii. Here, again, I have simply returned to the views of my old masters, and tried to support them by a large number of additional facts.

The argument in a long polemical book like this is necessarily involved, and also involves almost necessarily certain repetitions and iterations, and even an apparently inconsequent and parenthetical arrangement of the matter, all of which may become the subject-matter of criticism. To make the argument clearer I therefore propose to give here shortly a kind of epitome or syllabus of its contents.

In my former work on the Glacial Nightmare, I devoted 324 pages to a history of the various explanations and theories which have been forthcoming to account for the drift beds from the earliest time when they were first critically examined down to the period when, by the mere force of aggressive assertion, picturesque rhetoric and indomitable assurance, the champions of the glacial theory virtually drove all other champions out of the field. That survey was, I think, a fairly complete monograph, and I have not thought it necessary to again traverse any part of the ground it covers.

This historical retrospect was followed in the work mentioned by an analysis of the various theories which have been forthcoming to account for the postulated Ice Age and its doings, and in it I endeavoured to show that they are all more or less futile and untenable. To this subject I also devoted a large space. In the present work I have supplemented that survey by a large number of additional facts and arguments in order to try and analyse every possible theory that human ingenuity has produced to account for an Ice Age.

Such theories may be divided into those which treat it as a regularly recurrent event, dependent upon regularly recurring

causes, and those which treat it as a detached and substantive incident, and as the product of accidental and sporadic rather than of regularly recurring initiative.

The former are virtually limited to what are called " astronomical causes of an Ice Age," either those depending on the recurrence of certain favourable positions of the heavenly bodies towards each other or on similar causes when supplemented by physical or meteorological factors more or less ancillary to and associated with them. In my former work I devoted pp. 354-376 of chapter ix. to an examination of the astronomical theory of an Ice Age pure and simple, which had then been discarded by every notable astronomer, physicist and geologist as illegitimate. This examination was in fact rather an academic one, for the theory was then considered dead and had not a single champion.

A very potent and uncompromising champion of it appeared, however, not long after the publication of my book in the person of the Royal Astronomer for Ireland, whose official position and reputation gave his pronouncement exceptional importance. The following passages are a good measure of the unqualified language used by him as lately as 1892 : " The astronomical theory seems to offer the only intelligible explanation of the phenomenon in question " (*i.e.*, of the Ice Age). " In fact, it might almost be said that the astronomical theory must be necessarily true, inasmuch as it is a strictly mathematical consequence from the laws of gravitation. Perhaps it would be no exaggeration to say that even if geologists had not hitherto discovered the Ice Age from its records on the globe's surface, astronomers would have demonstrated by calculation that Ice Ages must have happened, and would even now be urging the geologists to go and look for their traces " (*The Cause of an Ice Age*, pp. 43, 44).

In view of the tragical end of the claim thus bravely announced, this language must sound inconsequent and disconcerting enough to all who care for the establishment of scientific truth by scientific methods, and who do not like to see the Philistine sneer and mock at popular science.

In spite of the virtually unanimous opinion of astronomers and physicists to the contrary, Sir R. Ball claimed that astronomical causes, unaided by any subsidiary or other agencies, are competent to initiate an Ice Age, and he claimed to have established that theory on an irrefragible basis. In addition to the countenance

of a certain number of supporters, who were neither mathematicians nor astronomers, he received the powerful help and adhesion of Prof. George Darwin. Sir R. Ball's arguments were soon assailed, however, and especially were they shattered by the masterly analysis of Culverwell. I have told the story at full length in the first chapter of the following volume. The result was that Prof. Darwin completely withdrew his support and declared Culverwell's arguments irreproachable, and Sir R. Ball himself eventually entirely abandoned his former position and fell back upon a modified form of Croll's theory to be presently mentioned. The only happy result of this resuscitation of a discredited and abandoned hypothesis was the rigid proof, of which I have given an abstract, by which Culverwell showed that not only Sir R. Ball's, but every form of the astronomical theory pure and simple professing to account for an Ice Age is, and must necessarily be, unsound. Thus the general consensus of opinion among astronomers, as laid down by one of their most distinguished colleagues, Mr. Meech, namely, "that the cause of notable geological changes must be other than the relative position of the sun and earth under their present laws of motion," was again firmly established.

In view, however, of the recent resuscitation of the opposite view by such a distinguished champion as Sir R. Ball, I have thought it well to analyse the arguments about it in detail, and to put on record the conclusive case against it.

While every astronomical theory of an Ice Age pure and simple is now universally treated as fallacious, there are some who think that such a theory when supplemented by certain meteorological elements still offers us hope of solving the greatest of geological riddles. This view was first put forward by Adhémar, a distinguished French mathematician, in his work entitled *Les Révolutions de la Mer*, published in 1840.

He did not deny that in consequence of the combination of the eccentricity of the earth's orbit and the obliquity of its axis (however these two elements within their strict limits of variation may vary) there cannot be produced any substantial alteration in the amount of sun-heat received annually by each hemisphere, namely, one-half of that received by the earth as a whole. He urged, however, that the occurrence of summer in one hemisphere and of winter in the other when the earth is nearest to the sun so affects the radiation of heat from the earth that the balance of

heat remaining to warm it is greatly altered, with a corresponding creation of a disparity in the climates of the two hemispheres, and he urged that this would be enough to create a glacial climate in one hemisphere and a temperate or warm one in the other.

He further argued that in consequence of the operation of the precession of the equinoxes there would be a transfer of this glacial and mild climate respectively, from each hemisphere to the other every 10,500 years, and a consequent recurrence of a glacial climate in each hemisphere after every interval of that length. He was the first really to urge on scientific grounds the regular recurrence of glacial periods in the geological past.

Adhémar's very ingenious notion was entirely based, as I have said, on the supposed effect of the differential radiation following upon the combined increase in the ellipticity of the earth's orbit and the inclination of the earth's axis to the ecliptic in distributing the climates of each hemisphere between its perihelion and aphelion journeys. This supposed effect was, however, quite a mistake. The arguments of Adhémar on the subject were sifted with great acuteness and convincing force by Croll, who was always at his best as a critic, and who showed that the position was quite untenable and the machinery quite inadequate to produce any glacial climate. I have condensed this proof in my *Glacial Nightmare* (pp. 380-384), and I have not thought it necessary to add anything in the following pages to what I there said, for the theory has no longer any supporters.

This is not the case with Croll's own theory. Croll was a very remarkable genius, endowed also with singular dialectical skill and dexterity, and, in addition to a considerable acquaintance with mathematics and physics, was an active member of the Geological Survey of this realm.

With Croll's name the glacial theory will always be more or less associated, for no one has championed it with greater skill and ingenuity. While discarding the astronomical theory of an Ice Age pure and simple, as utterly powerless and inefficient, he contrived to build up an exceedingly plausible alternative in which the automatic and necessarily recurrent elements of the discarded theory were retained and supplemented by an appeal to other and very intangible and difficult factors, namely, meteorological forces. While he urged that no Ice Age could directly follow from the initiative of merely astronomical causes, these causes might, he argued, set

in motion certain meteorological changes which were quite competent, if available, of operating in the fashion demanded and thus causing an Ice Age. To the elaboration of this position he devoted a famous book, *Climate and Time*, and to a defence against his critics a second work, *Climate and Climatology.* Whatever may be thought of the soundness of his conclusions there can only be admiration for his passionate fervour for his cause, and for the wonderful mental dexterity evolved in its defence. Croll, in fact, took captive a large number of influential geologists, notably in this country, where his colleagues of the Geological Survey, especially his Scotch colleagues, made up a formidable body of Janissaries who fought fiercely for him. At one time it looked as if his position was going to be generally accepted. Presently, however, critics sprang up in various directions, calling in question his facts and his logic. Especially was this the case with the meteorologists whose special province he had invaded, and some of the most experienced of whom scouted his arguments as utterly fallacious. Slowly and gradually Croll's position has been honeycombed and undermined, and now it would be difficult to find any geologist of any repute who sits confidently at his feet. Even those who believed in him most now write in hesitating tones about his theory or repudiate it.

In my *Glacial Nightmare* I devoted pp. 384-414 of chapter x. to an examination of Croll's theory. In the second chapter of the present work I have followed up this criticism with a large number of other arguments, partially my own but largely those of others, all of them of high competence and skill—Culverwell and Becker and Newcomb and Claypole, and especially by those contained in the masterly analysis of Croll's meteorological fallacies by Woeikof.[1]

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[1] In a more recent work by another very competent meteorologist, De Marchi, where there is also a dissection of Croll's views and methods, we read (the passage is in italics in the original): "*dal ponto di vista della climatologie e della meteorologia, al punto al quale sono attualmente questa scienza, l'ipotesi di Croll appare, sia nel campo dei principii come in quello dei fatti, assolutamente insostenibile*" (*Le cause dell' Era Glaciale*, memoir crowned by the *Lombardic Institute of Science and Letters*, p. 1(6).

In the *L. E. and D. Philosophical Magazine* (5th ser., vol. xli., pp. 274, 275) another very distinguished chemist and meteorologist, Prof. Arrhenius, quotes the view of De Marchi with approbation, and gives other reasons for discarding Croll's view as quite unsound.

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The most ominous, however, of all facts in regard to Croll's theory and its present shattered position is the attitude of two of his colleagues and former henchmen. In the article on geology, by Sir A. Geikie, in the tenth volume of the *Encyclopædia Britannica*, published in 1879, as also in his well-known text-book, published as late as 1893, Croll's glacial theory was adopted apparently without hesitation. In the supplementary article published in 1902 in volume xxviii. of the same Encyclopædia, Croll has been dropped out entirely, and nearly a column is inserted instead, devoted to Culverwell's crushing exposure of his arguments. This new pronouncement, *inter alia*, incorporates Culverwell's summing up of the theory as a " vague speculation with a delusive semblance of severe numerical accuracy but having no foundation in physical fact, and built up of parts which do not dovetail one into the other ". In regard to Prof. James Geikie, I have pointed out in the text how hesitatingly he now speaks of the views which he once held so firmly (see vol. i., p. 41). The case against Croll is, in fact, overwhelming, and, as I have shown (see *Glacial Nightmare*, pp. 414-416, and in the following work, vol i., pp. 80-87), the same arguments, with some additional ones, apply to his theory as modified by Murphy, who is another extinct volcano among those who have tried to rationally account for an Ice Age by combining astronomical and meteorological arguments.

One of the critical difficulties which Croll had to face in his theory was to start his ice machine. What really baffled him most was to find some cause by which he might secure that his glacial summer should not continually and continuously undo all the efforts of his glacial winter. Dr. Wallace, who in the main accepted Croll's faulty reasoning on physics and meteorology, tried to meet the critical difficulty here referred to by postulating certain notable geographical changes as necessary adjuncts to Croll's theory in order that his glacial machinery might be started, and that an accumulation of cold should be the result. Wallace's new departure was keenly opposed by Croll with his usual force and masterly vigour as a critic. The case against the former seems overwhelming, and has apparently been thought so by others, since, I believe, on this matter Dr. Wallace has no supporters whatever. I have supplemented my criticism of him in my former

work (pp. 417-421) by some additional remarks in the present one (see vol. i., pp. 89-98).[1]

Penck, a very ardent German glacialist, has adopted a theory not unlike that of Dr. Wallace, and, as I have shown (see vol. i., pp. 97, 98), marked by the same infirmities. All these hypotheses are more or less sophisticated by the frailties of Croll's own theory, which they partially adopt, and have additional ones of their own, and the best proof of their feebleness and ineptness is to be found in the fact that they are, so far as I know, merely the individual opinions of their ingenious authors, and have never, like Croll's own theory, dominated any powerful section of opinion.

What remains clear and plain after fifty years of hopeless effort is that the astronomical theory of an Ice Age, either in its simple form or as supplemented by an appeal to secondary agencies, has utterly failed to satisfy the conditions of a scientific hypothesis.

Sir A. Geikie in his last pronouncement on geology in the supplementary volume of the *Encyclopædia Britannica* closes his remarks on the present condition of the problem we are discussing with the ominous words : " *The failure of the astronomical theory to afford a solution of the problem of the Ice Age leaves geologists once more face to face with one of the most perplexing questions with which they have to deal* " (vol. xxviii., p. 634). This may be accepted as the final epitaph upon the astronomical theory in its various forms. It condenses what some of us have been urging with very small effect for a quarter of a century, and although belated the recantation is none the less welcome. I wish it had been accompanied by a statement that all that had been said on this subject in the *Encyclopædia Britannica* in

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[1] I will here add an additional criticism of Wallace which has reached me since the text was written, and which seems effective. Wallace in his *Island Life* (1st edition, p. 150) says : " It seems to me therefore quite certain that wherever *extreme* glaciation has been brought about by high eccentricity combined with favourable geographical and physical causes (and without this combination it is doubtful whether *extreme* glaciation would ever occur), then the ice-sheet will not be removed during the alternate phases of precession so long as these geographical and physical causes remain unaltered ". To this De Marchi replies : " Ma io non comprendo come queste cause (variazione degli alesei e delle correnti) potessero rimanere inalterate, quando anche l'emisfero australe fossi invasi dai ghiacci, e giundi rafforzali gli alesei de S.E. e indebiliti quelli de N.E. e il Gulfstream assai diminuto e anche interamente perduto per l'emisfero boreale, vi fossi per io reintegrato in tutti e in parte nella sua sede e nella sua copia primitiva " (*Le Cause dell' Era Glaciale*, p. 165).

volume x. must now be considered as obsolete and cancelled. It might possibly have saved some ingenuous student without either time or opportunity for original criticism from a pitfall. It ought to be conclusive now, however, to many to whom Sir A. Geikie is naturally an authority of the first rank. The utter collapse of every astronomical theory to account for an Ice Age necessarily, however, brings down with it some other notable speculations of a more directly geological character.

Croll was a great deal more logical and acute than most of his followers. He saw very soon and very plainly that if the astronomical theory of an Ice Age is to be sustained, it involves and necessitates certain definite geological corollaries which must be accepted with it, and which are in no way involved in those theories of an Ice Age which make it a mere sporadic and individual event and not a recurring one. He therefore devoted a great deal of ingenious sophistry to an attempt to show that the geological evidence does, in fact, support these corollaries, or, in other words, that the astronomical theory of an Ice Age is consistent with the geological record.

I have devoted a considerable space both in my former work and in this one to showing that this appeal to geology by Croll is entirely sophistical, and that far from the geological evidence supporting what are necessary adjuncts to any astronomical theory of an Ice Age, it is consistently opposed to them, and can only be brought into line with them by a distortion both of fact and inference.

I will shortly enumerate these corollaries. (1) As Sir R. Ball says: " It is the essence of the astronomical theory that a glacial epoch in one hemisphere shall be accompanied by a genial epoch in the other, and that after certain thousands of years the climatic conditions of the two hemispheres shall become interchanged. . . . Such fluctuations seem to have occurred again and again, *in fact, that they do so is a necessary consequence of the astronomical theory of the Ice Age.* . . . This is not a mere matter of ordinary significance, as it involves an absolutely vital point in the astronomical theory of the Ice Age. So much is this the case, that if it could be shown that the Ice Ages in the two hemispheres were concurrent, the astronomical doctrine would have to be forthwith abandoned " (see vol. i., p. 146). This is quite unassailable as a proposition, and what is the geological evidence. Sir R. Ball himself confesses

that it is of "a somewhat meagre character," which is putting it very mildly, and he elsewhere says, "it is difficult to imagine the nature of the evidence which would unerringly decide such a point". We have, in fact, no conclusive evidence that the periods classed as glacial in both hemispheres were either concurrent or consecutive. But all the evidence that is available, and apparently all the opinion that is of weight, is in favour of the conclusion that these periods were contemporary and not consecutive, and that in each case they represent the last phase in the world's history in each hemisphere. While in favour of the contrary view, I know of no evidence whatever save the *a priori* necessities of Dr. Croll's and Sir R. Ball's case.

I have shown this in my *Glacial Nightmare* (pp. 480-483) and in the following work (vol. i., pp. 146-150), where in addition to my own views I have quoted those of Agassiz, Penck, Wallace and Belt, all of them strong glacial champions, and three of them, oddly enough, namely, Penck, Wallace and Belt, champions of the astronomical theory of an Ice Age, and yet agreeing that the glaciation of the two hemispheres was, in fact, contemporary. They seem apparently quite unconscious of the crass inconsistency of this conclusion with their astronomical hypothesis.

The only fragment of argument adduced by Sir R. Ball in favour of his *necessary* corollary involves the postulate of a former cold climate in the tropics, which again he styles "*that critical doctrine in the astronomical theory of the Ice Age which asserts that the Equator must have been occasionally visited by a temperate climate*". In the use of the term "temperate climate" Dr. Ball has shown himself more modest than some other writers who have boldly postulated glacial conditions in the tropics in former times.

This extraordinary and most fantastic notion has actually had at one time the support of men as much regarded for their prudent reasoning as Agassiz, Wallace and Dr. James Geikie. Wallace and Prof. Geikie have, however, entirely recanted what they once said on the subject. If there be any serious person who still holds to the view of former temperate or more or less glacial conditions in the tropics (which, be it remembered, is a necessary postulate of the astronomical theory of an Ice Age), may I commend the case against the notion as I have condensed it from some very capable critics in my *Glacial Nightmare* (pp. 490-500), supplemented by that in the following pages (vol. i., pp. 149-155, and vol. ii., 32-41).

If the astronomical theory of an Ice Age in any of its forms be tenable, it clearly involves an alternate glaciation of each hemisphere. This, as Croll and Ball saw, is a perfectly necessary condition, and it is a necessary condition of no other theory known to me. Now, it is a very remarkable fact that in the southern hemisphere there is nothing to compare with the supposed traces of glaciation which are so widely spread in the northern one. The only part of the southern hemisphere where similar phenomena to those of North America and Europe are to be found, is in the southern part of South America. The supposed evidence of former general glaciation in Australia, Tasmania, New Zealand and the Cape, has entirely broken down, and I have very little doubt that if, and when, another edition of Dr. J. Geikie's *Ice Age* comes out, this part of his case will be presented in an entirely different way. I have collected and analysed the evidence on the subject at considerable length (first in my *Glacial Nightmare*, pp. 483-490, and at greater length in the following work, vol. i., pp. 155-171), and quoted the very conclusive results of the most recent critical examination of the problem on the spot.

Another necessary corollary of the astronomical theory of an Ice Age emphasised by its supporters (and not necessary to any other glacial theory) is the culmination of the supposed glacial effects at either Pole. Hence the interventions of the notion of *polar ice-caps* into the glacial theory by Croll, Torell and others. This particular postulate, however, has now ceased to trouble us. It has been surrendered by all, even the most extreme glacialists, and by no one more frankly than by Prof. James Geikie and Prof. Chamberlin.[1] The conclusive case against polar ice-caps I have tried to present in my *Glacial Nightmare* (pp. 500-518), and in the present work (vol. i., pp. 196-200).

This is not all: the fact that the supposed ice was not ranged round the Poles in the form of ice-caps, but was distributed in a quite unsymmetrical, aberrant and irregular fashion in regard to the Poles, and occupied, in fact, only one-half of the circumpolar

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[1] I will here quote an additional sentence from Dr. Geikie which I had overlooked: "Some geologists have supposed that the great *mer de glace* poured down upon Europe from the polar regions. But this is disproved by the direction of the striæ in the north of Norway, in the Shetland Islands and the outer Hebrides" (*Prehistoric Europe*, p. 205). What is strange to me is that Dr. Geikie has not seen that this admission is completely fatal to any astronomical theory of an Ice Age.

area in the northern hemisphere, is in itself quite conclusive against any form of the astronomical theory which necessarily involves a symmetrical arrangement of the effects of its action focussed about the Poles.

The different issues I have just referred to, although all ancillary and necessarily resulting from any astronomical theory of an Ice Age, do not however occupy a very large space in the literature of glacial geology which is largely remarkable for the issues it evades or ignores.

Two other conclusions remain. Both of them are similarly tied to the dominant theory I have mentioned, and loom very big in glacial literature. They oddly enough, however, retain their hold upon some who have entirely surrendered the astronomical theory. This is very curious since they were both originally introduced with the mere object of giving a geological support to that theory, and their continued retention is not only inconsequent but adds a very unnecessary burden to the weary jade which carries the glacial theory. It was Croll, with his acute vision, who first saw plainly that if the astronomical theory is valid, we must postulate not one glacial period only but many. To use his own words: "The theory demands that glacial epochs, in past geographical periods, should have been numerous and severe". Sir R. Ball says: "To Dr. Croll must be ascribed the credit of having pointed out that the remarkable succession of Ice Ages . . . was a necessary consequence of the astronomical theory of their origin ". Croll, therefore, set out to find evidence of a succession of glacial periods in the older geological ages, and some of his disciples have tried hard to support his efforts. The task, however, has proved too arduous. The facts are incorrigible and will not bend to the necessities of the great *a priori* postulate, and one by one the younger geologists have given up the quest. I know of no living geologist of any repute, save Prof. Geikie, who now holds that the older beds furnish evidence of a succession of glacial periods, or who would not deem it mere waste of time to try (as Croll tried) to form a chronology of such periods evidenced by the contents of the geological record, and dominated by the varying eccentricities of the earth's orbit. The notion still survives, however, in a kind of hazy way in many minds, and this being so I have thought it well to devote a considerable space to sifting and analysing the evidence. In my former work, this issue occupies

pp. 426-457 ; in the present one, vol. i., pp. 171-196, and vol. ii., pp. 12-32. The result of this analysis seems to me to show conclusively that Murchison and Lyell and Prestwich, Wallace, Leconte and Nordenskiold, all great names, which are supported by a crowd of others, were right in refusing to see any adequate evidence of a recurrence of glacial periods in the older rocks, and in limiting the phenomena, supposed to require the intervention of ice for its explanation, to one period only. As Prof. Leconte graphically says : " The evidence at present is overwhelmingly in favour of the uniqueness of the glacial epoch. This fact is the great objection to Croll's theory."

Croll not only postulated the recurrence of glacial periods in former geological times as a necessary postulate of the astronomical theory, in which he was quite logical and consistent, but he separated the most recent glacial period into several smaller and subsidiary periods which he claimed were divided from each other by the intercalation of a number of mild periods. These he styled interglacial. This, again, was postulated as a consequence and corollary to his astronomical theory of an Ice Age. A diminishing number of geologists still adhere to the notion of interglacial periods, and this on both sides of the Atlantic, but especially is the creed held among the Geological Surveyors of these realms whose memoirs unfortunately teem with references to them. The alleged interglacial character of certain beds is however purely imaginary and due to an entire misreading of the facts, and to purely *a priori* methods of reasoning. Interglacial beds ought, in fact, to be placed in the same limbo as the astronomical theory of an Ice Age which they were introduced to support. Here, again, the fact of there still being a number of adherents of the notion has led me to devote a considerable space to its analysis, and I have, I think, shown that it is quite futile.[1] This analysis may be found in my work on the Glacial Nightmare (pp. 458-478).

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[1] Let me here quote an apposite passage from Mr. W. Upham, an extreme glacial champion, which I had overlooked : " The formerly supposed necessity of predicating long interglacial epochs seems to me a misunderstanding. Instead, as Dana, Wright, Hitchcock, Lamplugh, Kendall, Falsan, Holst, Nikitin and many other glacialists believe, the Ice Age seems to me to have been essentially continuous and single, with important fluctuations but not of epochal significance, both during its advent and decline " (*Amer. Journ. of Science*, xlvi., p. 120).

In the following work I have treated the issue in a much more extended and complete manner. Chapter xi., vol. ii., pp. 42-131, will be found devoted entirely to the biological evidence on this issue.[1] In a later part of the volume I have similarly analysed the inorganic contents of certain of the drift beds which have been supposed to give support to the theory of interglacial periods, and have shown how they completely fail to do so, and how their witness has been misread (see chapter xv., vol. ii., pp. 318-328).

There remains one other corollary of the astronomical theory advanced by Croll, and now much out of favour with virtually all glacialists, namely, his calculation of the actual age of the great glacial period as deduced from his periods of excessive eccentricity and which put back that age to a portentous distance. I have examined and criticised his reading of the facts in my *Glacial Nightmare* (pp. 458-460), and at greater length in the following work (vol. ii., pp. 200-210), and shown that the facts themselves cannot, in any way, be made consistent with the necessities of the astronomical theory in this behalf.

This completes the survey of the available geological evidence which has been at various times adduced in favour of that theory, and which must necessarily be equated with it if it is to stand. As will be seen, the evidence at all points, in fact, refuses to be so equated, and it thus completes a discomfiture which had already been effected by the astronomers and the meteorologists. Here, again, it is well to remember how positively and confidently Sir Robert Ball took the very opposite view so lately as 1892. Speaking of the astronomical theory of an Ice Age, he says : " *We have shown that the astronomical conditions are so definite, that astronomers are entitled to direct that vigorous search be instituted on this globe to discover the traces of those vast climatic changes through which astronomy declares that our earth must have passed. . . . It seems impossible to deny that the labours both of geologists and astronomers have, in this case, both illustrated and supplemented each other. The geologist has supplied the evidence which the astronomer wanted ; the astronomer has supplied the explanation which the geologist wanted.*"

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[1] In this chapter I have also tried to examine and settle, with such care as I am capable of, the actual horizon of the mammoth and palæolithic man in reference to the drift beds, a polemic upon which I have written much elsewhere.

The fact is that at every point where geology has been appealed to, to sustain the astronomical theory of an Ice Age, it has refused to be a friendly witness and has spoken out unmistakably on the other side.

Alternate glaciation at either Pole, tropical glaciation, polar ice-caps, the portentous chronology of the drift period, recurrent ice ages, interglacial periods, these have all been discarded by the great majority of geologists, and are no longer tenable under any pretence. They were invented by Croll in his strenuous efforts to maintain and support *his* astronomical theory. With the disappearance of every form of the astronomical theory, there is no longer any need to cumber our geological handbooks with a mass of special pleading in regard to them, which every discovery makes more incredible, and which has been a sore burden to many hapless students. I wish it were as easy to cancel them from a considerable number of *Memoirs of the Geological Survey* where they ought never to have appeared, since they are mere speculations and not descriptions of actual facts.

Having examined those theories which make the glacial conditions recurrent, let us now turn to those which profess to account for the glacial period as a unique event entirely detached as to its inception from astronomical causes. These theories are very many, and they seem as inexhaustible as is the perversity of the human imagination. I have examined a great many of them : all of them, indeed, which have come my way while I have been writing this book. First are some theories involving more or less astronomical conditions but not implying recurring events, namely, those built up by some writers by postulating a shifting of the earth's axis as a possible cause of climatic changes. This notion has taken two forms. Some people have based it on the supposed possibility of a quite abnormal alteration of the inclination of the earth's axis to the ecliptic which, as we have seen, is strictly limited in amount by the astronomers. To this view Drayson and Belt and others have given countenance in spite both of the mechanical and physical absurdities it involves (see *Glacial Nightmare*, pp. 363-370, and the following work, vol. i., pp. 10-17 and 99-101).

The mechanical and physical objections to any alteration of the obliquity of the ecliptic on a sufficient scale being manifest, it has been attempted to reach a corresponding result by supposing that

the axis of the earth has itself been considerably shifted within the sphere, so that the two Poles have now a different emplacement on the earth's surface to what they once had. This view has been presented in two ways. According to one notion, which has been supported by my friend Mr. Oldham and others, the earth has in some inexplainable way, and in spite of its great equatorial protuberance, succeeded in changing the locus or position of its axial pivot, and thus shifted the axis about which it revolves to a large extent, and this without completely destroying itself and being utterly broken to pieces. According to another notion propounded by Sir John Evans, and in a more fanciful way by M. Peroche, it has done this by a movement of its postulated solid crust over its liquid nucleus. The theories in question have been critically examined by a number of mathematicians and physicists of the first rank, such as Lord Kelvin, Prof. George Darwin, Prof. Twisden, Prof. Avery, Dr. Croll, the Rev. E. Hill, etc., who have all concurred in the conclusion that they involve a fantastic notion inconsistent with the laws of physics and terrestrial mechanics; while geologists such as Sir A. Geikie, Prof. Haughton, Prof. Jukes and Dr. Gregory have shown that even if possible the changes in question would completely fail to meet the geological difficulties. The whole question of the possibility of a shifting axis and of its possible results has been stated at length in my *Glacial Nightmare* (pp. 341-354), and in the following work (vol. i., pp. 101-112).

Another series of hypotheses are dependent on the notion that the earth has at various times in its history received a varying amount of heat from sources external to itself. All these notions involve a general lowering or raising of the temperature of the world as a whole, or of one of its hemispheres as a whole. I have examined a large number of them, and they seem to me to compel one verdict and one verdict only. They every one traverse in some way or other the laws of physics and the possibilities of Nature as they have been hitherto tested, or else they fail entirely when correlated with the facts of experience. Those who wish to see what has been said in regard to them, not by myself only but by many other critics more competent than myself, would do well to turn to the analysis I have made of them which I believe to be the most complete in existence. It is contained partly in my *Glacial Nightmare* (chapter ix., pp. 325-340) and partly in the following work (vol. i., pp. 112-143).

I may say that to my mind nothing could be more clear and convincing on these particular issues than Sir Robert Ball's pronouncements, which I have largely used, since they seem to me better than anything I could say on the subject myself, and to stand out in marked contrast with what he has said in defence of the astronomical theory.

Apart from the many objections of various kinds against the theories of an enhanced or a diminished supply of heat from sources external to the earth collected in the text, there is one mentioned by Dr. Wm. Wright in his *Man and the Glacial Period* which I should like to repeat here : " The accumulations of ice during the glacial period," he says, " were not determined by latitude. In North America the centre of accumulation was south of the Arctic circle, a fact which points clearly enough to some other cause than that of a general lowering of the temperature external to the earth " (p. 306). The asymmetrical arrangement of the alleged glacial phenomenon, to which should be added a reference to their complete absence in that half of the northern hemisphere covered by the area from the White Sea to the Copper Mine River, and their quite sporadic distribution in the southern continent, exclude, in fact, all cosmic causes as incompetent to meet the difficulty.

There remain the causes, purely mundane and limited, in their inception to the earth itself. It was long a favourite notion that secular changes of climate on the earth might perhaps be due to its gradual cooling, but this process tends all in one direction and ought to be supported in the geological record by evidence of a gradual and continuous cooling of the earth's surface in time, which is not the case. It would, again, affect the whole earth together, and not account for the aberrant distribution of the drift, which is another reason against it. Other and more cogent reasons, and notably the analysis of the case by Lord Kelvin, may be found reported in my *Glacial Nightmare* (pp. 327-329) and in the following work (vol, i., p. 128).

Another set of theories deals with supposed possibilities of change in the constitution of the atmosphere and a consequent variation in its blanketing effects upon the world's heat. This is supposed to have been brought about by the variation in the quantity either of moisture or carbonic acid in the atmosphere. Croll long ago said of such theories that they overlook the fact that the same

heat-intercepting element in the atmosphere which prevented the escape of terrestrial heat would also shut off the solar radiation, and *vice versâ* when the blanketing layer was reduced, so that what was gained in one way would be correspondingly lost in another. Not only so, but it must be constantly remembered that an Ice Age needs a greater evaporation as well as a more powerful condenser, and that any blanket that intercepted the sun's heat would diminish the amount of evaporation and thus reduce the snow-fall. Such a cause, again, would be very difficult to equate with the very singular and aberrant distribution of the so-called glacial deposits. Prof. Chamberlin allows that the cause he now leans upon, namely, an intermittent supply of carbonic acid in the air, would act upon the atmosphere as a whole, whereas the effects to be explained are local. As Mr. True neatly puts it: "How could the carbonic acid be extracted from the atmosphere over the area of the drift region and leave the rest of the earth undisturbed in its proportion of carbonic acid? . . . The glacial region, though extensive, was a local phenomenon; whereas the assumed abstraction of carbonic acid would be a general phenomenon" (*The Cause of the Glacial Period*, p. 125). I have further criticised this theory of Chamberlin's in my *Glacial Nightmare* (pp. 329, 330) and in this work (vol. i., pp. 132-134).

It may be a subject of comment that no mention is made in the following pages of a theory recently propounded by my friend Mr. Harmer in a paper in the *Quart. Journ. Geol. Soc.* (vol. lvii., pp. 405-476) on "The Influence of Winds upon Climate during the Pleistocene Epoch". This paper apparently revived some scanty hopes among the prophets of the creed, that at last a haven of rest had been discovered where the weary glacialists might comfortably repose, in confidence that their great hypothesis had at last been justified by a rational cause. As a matter of fact Mr. Harmer's paper does not touch the question I am discussing at all. He does not profess in any way to explain the glacial theory. His very words are: "*I do not venture to express any opinion as to the cause of the glacial cold*" (*op. cit.*, p. 431). Again, speaking of his theory, he says: "The average temperature of the northern hemisphere during the pleistocene period was *from some hitherto unexplained cause lower than that of our era*" (*ibid.*, p. 472).

What Mr. Harmer proposes to show by his paper is, granting a glacial period, what effect it might possibly have upon the dis-

tribution of the areas of high and low pressure over the earth, so as to account for certain secondary oscillations of temperature during the glacial period itself which he thinks probable.[1]

That is quite a different issue, a very speculative and difficult issue, as its author allows, and it involves, besides some very doubtful meteorology, at least one postulate which few people will be found to support, whatever their other views on the glacial theory may be, namely, that the glacial conditions in North America and Europe were not contemporary. This, however, must not detain us, since Mr. Harmer does not professedly touch our problem at all.

The difficulty of meeting the case created by the unsymmetrical and aberrant distribution of the drift beds has led a considerable number of geologists, including, *inter alios*, De Lapparent, to fall back upon Lyell's views in regard to the possibilities of great climatic changes having ensued from changes in the distribution of land and water on a great scale on the earth's surface.

It seems to me that here Prof. James Geikie's arguments are conclusive in regard, first, to the impossibility of creating anything in the shape of a glacial period by means of a mere redistribution of geographical areas, and, secondly, to the conclusion that if it were possible, the evidence is very strong that there have been no such geographical changes as Lyell's theory would demand in pleistocene and later times. I regret that Mr. Geikie should have cut down the admirable arguments on this issue occupying eight pages of the second edition of his *Great Ice Age* (pp. 86-93) to a mere page and a half in the third edition (pp. 791, 792). What I have had to say on this theory will be found on pp. 137-140 (vol. i.).

Another and more popular notion which still has many adherents is the so-called *epeirogenic* theory, which postulates a great elevation of the land-areas in high latitudes in former times as an efficient cause of an Ice Age. This view is much favoured in America, where it has been especially urged by Mr. Warren Upham. In this country it was strongly pressed by Prof. Hull in his paper on " Another Possible Cause of the Glacial Period "

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[1] The paper is really a meteorological and not a geological one, and would have been a great deal more at home in a meteorological journal than where it is.

(1898). I have analysed it further in this work (vol. i., pp. 135-137, and vol. ii., pp. 2-8), and shown how it entirely fails to meet the necessities of the case. I would like to add to the views referred to in the text a reference to Dr. Wm. Wright's analysis of the American evidence on the question contained in pp. 410-415 of his *Ice Age in North America.*

This completes the general survey of the various theories to account for an Ice Age hitherto proposed as they are discussed and analysed in detail in the following work.

The conclusion that all these theories are ineffective and insufficient is not mine merely. It is that of almost every one who has paid much attention to the subject. Writing in 1893, the distinguished Belgian geologist, De Lapparent, in speaking of the causes of the Glacial Age, talks of " l'impuissance notoire de toutes les explications jusqu'ici proposées ". M. Martins, a great glacial champion, says : " L'ancienne extension des glaciers est un fait ; la decouverte des causes qui l'ont produite sera l'honneur des futures générations scientifiques ". Heim, an authority of the first order, says : " Bis zur Stunde müssen wir eingestehen, dass wir die tiefere Ursache der Eisziet noch nicht kennen, so vieilerlei verschiedene Gründe uns denkbar erscheinen mogen. Die Lösung auch dieser Frage ist der Zukunft überbunden." Prof. Bonney says : " The low temperature which undoubtedly prevailed during the glacial epoch has not yet received any satisfactory explanation ". Chamberlin describes the riddle as " remaining unsolved ". Dr. G. F. Wright speaks of " the complicated problem which has so far baffled us ". Last, and not by any means least, comes the doyen of living English geologists, Sir A. Geikie, who once accepted and adopted Croll's explanation, and who, as we have seen in his very latest pronouncement, not only rejects that explanation, but in doing so says that " it leaves geologists once more face to face with one of the most perplexing questions which they have to deal. At present," he adds, " no satisfactory explanation of it," *i.e.*, the Ice Age, " has been offered."

This confession is made with singular *sang-froid* and complacency by its authors, who may be taken to voice the best and most responsible geological opinion. I confess I cannot myself be so complacent. To me it involves an absolute departure from scientific methods, unless accompanied by a surrender of a great deal more. It becomes a purely metaphysical pastime to appeal

to a cause which cannot be explained and which has no support from experience. Such a cause is as purely transcendental and imaginary as most of those against which Bacon's philosophy protested.

To postulate a glacial period in order to co-ordinate and explain a great number of physical facts is legitimate when we have shown the possibility of such a cause and how it could be produced and worked, but it is not legitimate when every effort has hitherto failed to so explain it. In so postulating it we are departing from true induction and building a house on the shifting sands of an imagination running riot. Let it be remembered that for sixty years, during which this theory has been current, the ingenuity of some of the most sharp-witted men that have ever lived has been exercised in manifold ways in trying to find a rational explanation of it. Hitherto, as we have seen, every attempt to show how such a glacial period could come about, or what could cause it, has failed. The appeal is, therefore, not to a rational cause—a cause whose possibility has been verified—but to an unverified hypothesis outside of the realm of science. I should like to especially call my reader's attention to pp. 8 and 9 of volume ii. of the following work, in which I have emphasised this view more strongly.

This is not all. It is not merely the admitted failure of every attempt to rationally explain the possibility of a glacial epoch which faces us. There is a further question quite as critical which has been most unaccountably ignored by the great mass of glacialists. It has been assumed by them that if we can only secure an Ice Age (even if we postulate it without giving any rational reason or cause for it) everything else will naturally follow : that all the intricate phenomena associated with the drift deposits may quite confidently be attributed to its handiwork ; that omnipotent ice (an exhaustless store of which will be thus obtained) is capable of bearing any burden we choose to put upon it, and that there is no necessity to test the capacity of our machine before appealing to it. It seems to me that until we have so tested it the appeal in question is no more scientific and legitimate than the old-fashioned appeal to a *Deus ex machina* to solve every difficulty. It is absurd to attribute quite portentous results which on this scale are entirely outside the range of human experience and of the current operations of Nature, and due to a

cause which we cannot directly examine or experiment upon until we have first made a serious attempt to equate it with the effects which we would trace to it (see vol. ii., pp. 9-12).

It has been my continual complaint for years (Cassandra's voice I know it has been) that the glacialists who appeal to a transcendental ice period have never attempted to show what is the first element in the problem, namely, that ice is capable of the effects which they deduce from it. Whole libraries of books and pamphlets have been written on the subject of the glacial theory, and I make bold to say that in hardly one of them will there be found any discussion or consideration of the nature and capacity of ice as tested by observation and experiment.

It is nothing short of incredible, for instance, that in Prof. Geikie's last edition of the *Ice Age* (the bible of the extreme glacialists, written by one who recently presided over the Geological Survey of Scotland, and who is now the most distinguished geological professor north of the Tweed), a work which runs to over 600 closely printed pages, barely two pages and a half should be devoted to an examination of the prime postulate of any glacial theory, namely, the dynamical capacity of ice.

Even this very scanty notice is so jejune and trifling and elementary that it is not in the least illuminating, and yet nothing can be plainer than that (unless ice is competent to do what it *is taken for granted* all through the book and without the slightest warrant that it can do) the whole argument of the work is a mere rope of sand. The fact is that like some monster relied upon by a child to solve any of its imaginative crusades against evil-doers, the ice monster is postulated as capable of any performance. The worst of it is that the child's monster does not impose on educated people, while this one which is equally imaginary being supposed to be attested by so many great men does most effectively impose on many of Dr. Geikie's readers who believe that he appeals to real ice, that is to frozen water, and do not know that his ice is a mere patent mixture invented by Croll and adopted by himself, and endowed by them with all kinds of imaginary qualities like the universal " cure-all " of some empyric.

For the most part those who have written on the subject of the Ice Age have had no experience whatever of ice either in the laboratory or in glaciers. Many of them had never seen a glacier at all when they wrote reams of absurd speculation about glacial

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action. The ice they write about and think about and postulate is not frozen water but an hypothetical ice which exists in the imagination only, and whose qualities therefore are purely imaginary. Hence it is easy to appeal to it for any purpose under heaven. This has been especially the case with the English official geologists who have written on so-called glacial deposits. Nowhere in the literature of any science will be found, I am certain, so much unverified and unverifiable speculation as in their works and in the hundreds of pages in the *Quarterly Journal of the Geological Society* devoted to glacial matters.

Croll, whose insight was phenomenal although his reasoning was often faulty, saw very clearly that with such ice as we know, which moves merely under the influence of gravity as other viscous substances move, the stupendous results he and the other glacialists demanded from it would not be forthcoming. He therefore postulated the peculiar ice which I have referred to with a very extraordinary molecular structure, and with a still more extraordinary molecular motion, and which, being quite imaginary and transcendental, could be called upon to perform any legerdemain its author demanded from it. When his hypothetical ice was examined critically by clear-thinking physical experts and notably by my friend Mr. Teall, now Director of the Geological Survey, it was shown to be a mere mass of absurdities. Teall's criticism upon it, which was absolutely crushing, was published in 1880. In this criticism he speaks of Croll's molecular theory of ice structure and movement as "an absurd speculation," as involving "a pernicious form of reasoning," and as being "as far removed from science as the speculations of the schoolmen" (see vol. i., pp. 388-390).

It ought to be remembered that this "absurd speculation" was adopted *en bloc* by Prof. Geikie in the first and second editions of his *Great Ice Age*, the latter of which was published in 1877, and it is upon this theory of the structure and capacity of ice that he proceeded to build the vast superstructure he subsequently presented us with. With such purely imaginary ice any imaginary function or effort is conceivable, and upon it any superstructure can be built, however fantastic. In the third edition of his book, published in 1894 (a year after the appearance of my *Glacial Nightmare*, in which I had analysed Croll's theory and shown it to be quite untenable), Prof. Geikie entirely cut out the seven and

a half pages which had previously contained his epitome of that theory. He did not say a word about my book or its contents in his new edition, which was perhaps singular. He merely substituted (without any explanation or warning of any kind) for the long rigmarole about Croll's transcendental ice, which he had meanwhile cancelled, the page and a half of new and almost puerile matter I have already criticised, in which Croll's name does not occur, and in which an entirely different theory of ice motion is set out. This new theory is not only quite inconsistent with Croll's, but what is more, it cannot be equated by any process with the effects Croll deduced from his own special ice. The abruptness of the change is really too absurd. The short paragraph Dr. Geikie devotes to his new master in lieu of the seven and a half pages devoted to his old one begins with the words : " The physical research of late years has apparently established the truth of Forbes's theory ". Quite so. A good many of us had said this nearly a quarter of a century ago. The statement is no doubt perfectly true and just. Its long incubation and sudden birth are none the less curious and edifying. I refer to it not because I have any objection to Prof. Geikie entirely and suddenly changing his geological faith on a most critical factor in the glacial theory without a word of warning to his readers, and adopting the view for which some of us had long previously fought, but because it has landed him in an extraordinary quagmire.

Croll's ice, his imaginary transcendental ice, was quite a proper foundation upon which to build a work like the *Great Ice Age*. He had, in fact, created it with the special qualities required for such a position ; but Forbes's ice, real ice, the ice of glaciers and presumably of ice-sheets, is a very different matter, which cannot be made the foundation for such a stupendous induction without a great deal of explanation not forthcoming in this book. Chapter xiii., pp. 519-593, of my *Glacial Nightmare* is devoted to a minute discussion of glacier ice, its origin and properties, and especially to the theory of glacier motion. The evidence is there adduced, upon the credit of which it is now generally accepted, first, that glacier ice is essentially different to pond ice or ice artificially produced by freezing water in the laboratory, and is granular in structure, and, secondly, that the motion of ice is, as Forbes always declared it to be, ordinary viscous motion, and has nothing occult and mysterious about it.

It seems to me that if and when Prof. Geikie brings out a new edition of his work he will do well to devote a considerable portion of it to showing how a substance like ice, which merely moves as any other viscous body does, can possibly act as he postulates that it acted when it did the dynamical work which he attributes to it. Perhaps in addition to chapter xiii. of my *Glacial Nightmare*, he will do me the favour of reading the additional matter on the physics of ice contained in the following work (vol. i., pp. 378-403). It is possible, however, that he may, when his new edition comes out, reconsider his perhaps too hasty adherence to Forbes, for another champion of transcendental ice has turned up in America in no less a person than Prof. Chamberlin. I have described this new departure into metaphysical geology in vol. i., pp. 394-397, where I have referred to an excellent antidote to it by Mr. I. C. Russell, and have also analysed its quite scholastic and mediæval methods and character, and shown how it entirely departs from all inductive experiences of ice, and how, like Croll's phantasm, it is merely a necessity of an impossible theory of *ice action.*

To revert: it is upon the ice theory of Forbes, which now holds the field, that I profess to stand and have always stood. That theory seems to me absolutely inconsistent with the kind of work attributed to ice by the extreme champions of the glacial theory. To be a little more concrete, I would urge that ice, being a viscous body, when armed with suitable tools in the shape of stones, can polish and burnish and in some measure erode, but cannot, except under very exceptional and peculiar conditions and in very limited areas, excavate and dig. This view is now very widely and generally held, not only on *a priori* grounds, but as the result of the examination of a large number of instances of supposed excavation by ice.

The notion that ice can excavate was first urged by Dana, who attributed the digging out of fiords to glacial action. This claim was extended to ordinary valleys by Tyndall, to cirques by Gastaldi and Helland, and to lakes by Ramsay, while Belt boldly extended the operation to all the great features of the country in his astounding phrase: " Not only the valleys and fiords of the North, but the great plains of Europe and Asia were produced by it," *i.e.*, by ice. Among those who have favoured this excavating view in a greater or less degree have been

J. Geikie, Helland, Stark, Logan, Haast, Jukes, Wallace, Brown, Murphy, Newberry, Ward, K. Steenstrup, Gastaldi and Penck, names chiefly, it will be noted, of an elder generation of geologists, several of whom are dead, and largely representing the views of thirty years ago. It is a curious and rather ominous fact that among these champions of the excavating power of ice, the arguments of two, Tyndall and Ramsay, are mutually destructive. On the other side are ranged a larger and much more influential list, including nearly all the young geologists. They comprise, *inter alios*, Agassiz, Murchison, Lyell, Bonney, Whymper, Duncan, Judd, Marr, Desor, Escher von der Linth, Falsan, Chantre, Collomb, Favre, Gurlt, Heim, Kjerulf, Pettersen, Martins, Mojsesovics, Reclus, E. Richter, Rütimeyer, Stoppani, Whitney, I. C. Russell, W. Hamilton, J. W. Taylor, J. Ball, Oldham, Irving, Freshfield, Carick Moore, J. W. Buchanan, and Goodchild.

For those with whom authoritative names do not weigh but who are impressed by the arguments of the best observers, I have condensed what seems the overwhelming case against the excavating power of glacier ice, including the alleged excavation of valleys, fiords, cirques and lakes, in my *Glacial Nightmare* and the following pages. In the former work this criticism is to be found on pp. 598-654, and in the latter in vol. i., pp. 416-458.

It is not in regard to its excavating capacity alone, however, that the impossibility of equating the capacity of *real ice* and the supposed results of its handiwork has arisen, and in which the opinion of a generation of geologists which is dying out seems to need revision. A more important and far-reaching difficulty which the glacial champions have to face is the proved incapacity of glacier ice, as of any other viscous body, to travel over the enormous stretches of more or less level country, and up and down long hills, as it must have done if the glacial theory is to become the final and effective explanation of a large part of the drift phenomena. Ice, so far as it is known to us by experiment or in the field, may move in two ways. If it be planted on a piece of level ground, *i.e.*, under the conditions postulated in ice-sheets, it may be pushed along *en masse* like any solid object which has a sufficient thrust behind it.[1] *A fortiori*, it can move in this way if

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[1] By the way, Sir R. Ball apparently thinks that the principal motion of a glacier is of this kind, whereas it is very doubtful indeed if it accounts for anything but a fraction of its motion. His comparison of the scouring and

it lies on a slope. The real difficulty, of course, is to secure any sufficient thrust when the ice is on level ground. This is plain, but it is a very misleading fact, and it has led many of the glacialists into a difficulty. It has been argued by them that, granting a sufficient pressure from behind, there is no limit to the quantity of ice that can be thus pushed along *en masse.* This is a great delusion. Like every other more or less solid substance ice will crush under a certain weight. Precisely the equivalent in pressure of this crushing weight will be equally effective if applied by way of thrust, and it has been shown that as a matter of fact it is not possible to pile up a mass of ice to an indefinite height, or to force a mass of ice of greater length than about seven miles along a level surface by any pressure, however obtained, without its crushing, and without, therefore, the thrusting force being dissipated. This is a critical difficulty which never occurred apparently to the extreme glacialists, who have had a supreme confidence in Croll's privately patented ice, and it clearly shuts out from the operation of ice when moving *en masse* and when subject to mere thrust, movements extending over hundreds of miles of level country, and, *a fortiori*, the climbing of ice over a long, rolling country or mounting up long slopes.

Secondly, ice as a viscous substance moves entirely by the influence of its own gravity, under which influence, when it is piled up on a level surface in the shape of a mound, its upper layer will roll over its lower ones until the slope of its surface is reduced to that of equilibrium.[1] This movement is, of course, a differential one. It is greatest in its upper layers and gradually diminishes as we near its bed. Its lower layers are virtually quiescent, like the lower layers of deep, slow-moving rivers probably are. It has, in fact, been experimentally shown that in glaciers, when on a slight slope or on level ground, the motion, which gradually diminishes with the slackening of the slope, is almost virtually confined to the upper layers, and that the lower layers are almost, if not quite, quiescent. *A fortiori* would this be the case with great ice-sheets.

denuding effects of a viscous body like ice with what might happen if one of the pyramids of Egypt were set in motion is preposterous.

[1] Here, again, Sir R. Ball is most cryptic and enigmatic in his language. What can he mean when he speaks of "the glacial sheet becoming gradually thicker until the pressure of the superincumbent mass forces the *subjacent* layers into tardy motion"?

I know of no other motion of which glacier ice is capable than the two here referred to, and, this being so, I cannot understand the temerity with which it has been urged that the products of the drift have been carried by ice-sheets, which, *ex hypothesi*, chiefly culminated in low lands, for hundreds of miles of level or broken country, often up and down hills, and sometimes right through great hollows, like the Baltic, the North Sea and the Lake of Geneva, or over mountains, athwart the lines of least resistance. The process seems to me absolutely transcendental when tested by the ways and capacity of real ice, and it is quite preposterous that it should have been postulated without any attempt of any kind to show its feasibility or consistency with the proved qualities of what is a viscous mass of frozen water and nothing else.

Thirdly, the notion that there can be transverse or divergent currents in great masses of ice crossing each other at different levels and travelling in different directions, which it has been thought necessary to invoke to explain some of the glacial phenomena, seems to me perfectly fantastic and contrary to every law of mechanics and physics, and to be only conceivable in the very exceptional conditions specially described by me in the text.

Fourthly, the notion that great ice-sheets, pressing down upon their beds with a pressure of many tons to the square foot, can break up and then dig out portions of their own beds seems to me as unthinkable as that Nelson's monument should do the same. If it be possible, then assuredly the *modus operandi* ought to be shown us instead of our being left with a mere *obiter dictum*.

Sir R. Ball speaks quite gaily about this incomprehensible process. Thus he speaks of an ice-sheet : " Ponderous in mass and irresistible in capacity, by whose influence fragments of the living rock are ripped from their bed and crushed to pieces " (*Cause of an Ice Age*, p. 14). Again : " At the bottom of the blue and solid river pieces of stone are occasionally wrenched from the rocky base " (*ibid.*, p. 6). " It is in the power of a glacier to rip stones from their bed " (*ibid.*, p. 9). How is this extraordinary mechanical legerdemain to be compassed ?

The limitations to the power and effectiveness of glacier ice as a dynamical agent contained in these paragraphs are elementary ones. There has been an unceasing demand for many years past from some of us, directed to the glacialists, to justify their ignoring them. It has been absolutely without response, and we have

literally had nothing offered us in the shape of explanation except the iteration of fables and fantastic phrases. Not one of them has attempted to seriously show that glacier ice is capable of such work as they attribute to it so glibly. Some of them have, in fact, confessed their inability to equate the position as it stands, and boldly allow that it is not reasonable to expect it to be so equated. They tell you that it is not reasonable to apply the lessons of our comparatively meagre but real glaciers to their portentous but purely imaginary ice-sheets, that is to say, they have remitted us back further and further from the realms of reality where we can experiment, to the fantastic world beyond the north wind where the fairies live, and where we cannot follow them any more than we can follow Alice into Wonderland.

The question as a serious issue is surely to be tested, not by transcendental, but by real ice and its capacity, and on that basis alone. I have tried to test it in chapter xv., pp. 659-684, of my *Glacial Nightmare*, and in vol. i., pp. 398-426, of the following book.

It is not only the incompetence of ice to do the gigantic porterage evidenced by the drift beds in any fashion consistent with their present distribution that meets us when we test our problem critically.

As we have seen, it is now clearly established that the organic contents of the drift beds, which were once quoted as evidences of a glacial climate, are all derivative. So far as the evidence can be sifted this is certainly so, and the position is admitted by some extravagant champions of the glacial theory. This excludes the supposed testimony of the organic world to an Ice Age. The presence of foraminifera in a large number of the so-called glacial clays which have been examined microscopically is again an especially serious objection to their being ice deposited.

It is not only the organic contents of these beds of which this may be said, however. It is becoming plainer every day that the boulders, the gravels, the clays, and the sands and loess which form the drift beds had been fashioned long before the date of their ultimate deposition ; that they were not in any way the product of the time when the drift beds were distributed, and it is a great infirmity of nearly every discussion of the glacial problem that this particular difficulty has, in no case that I know of, been discussed or analysed. If all these materials be, as I most firmly believe them to be, products of a much older denudation than

the so-called glacial age, then all the ingenious writing there has been on the subject of the rounding of boulders, and the making of boulder clay or loess by ice action during the glacial period, falls to the ground. These inorganic contents of the drift, like its organic contents, are, in fact, derivative and not contemporary. To this view I am a very devoted adherent, and to supporting it I have devoted a considerable space in the following volumes in which I have tried to sift this forgotten, or rather neglected, problem, a problem which stands at the very threshold of a really scientific analysis of the drift.

To it I have, in fact, devoted chapter xiii., pp. 267-320 of vol. ii., where I venture to think a very unexpected mass of evidence will be found, showing unmistakably that the so-called glacial sands, ✦ clays, shingles and rounded boulders are, in fact, derived from tertiary beds or even older strata, and so far as we can judge are as foreign and derivative as their organic contents.[1] This being so, they cease to have any value whatever as indices of glacial action, unless it be glacial action in far-off tertiary times which we have nothing to do with here and in which no one believes.

This is not all: the same reasoning seems to apply to another feature of the so-called glaciated lands, the one, in fact, which has occupied the entire vision of some enthusiastic glacialists, I mean the rounded *moutonnée*, polished surfaces of rock which occur so frequently in high latitudes, and which are in most cases covered and disguised by great masses of drift. I have discussed these surfaces in vol. ii., pp. 320-324, and will now, in regard to the whole of this special issue, quote the concluding paragraph of the chapter in which I have summed up my case : " A general and most important result from these facts and arguments is, that whatever it was that mixed and distributed the soft surface beds of the drift, *it had no part in manufacturing the ingredients out of which those beds were fashioned*. These ingredients were already fashioned and ready to its hands. The sands were already there in the shape of Crag and Bagshot and Reading sands. The clays were there in the form of Chillesford clay, London clay, Oxford clay,

[1] Again, in regard to the so-called till or boulder clay, it has long been argued, and every observation confirms the conclusion, that nothing like it is being made by glaciers anywhere, while it has all the features of water-deposited material and none of those of moraine material (see *Glacial Nightmare*, pp. 685, etc., and 777-779, and the following work, vol. ii., pp. 295-297 and 302-309).

and Kimeridge clay, etc. The polished flint and quartzite pebbles were there in the form and shape we now find them. The chalky, oolitic and liassic rubble was there in the form of rubble, and the far-transported crystalline boulders were rounded and ready for transportation ; while the rounded, polished and *moutonnéed* and whale-backed rocks which attract us all wherever they occur *in situ*, and have converted many sceptics to the glacial faith, were there, and all, so far as we can see, belong to an older[1] horizon altogether."

If this be so, and it seems to me that after the very strong case I have stated, the burden of proving the contrary rests with the ice men, then we have virtually only two phenomena left for consideration which may be deemed more or less contemporary with the distribution of the drift, namely, the striation of the rock surfaces and boulders, and the formation of the angular pebbles, boulders and erratics which occur sporadically in many drift beds and entirely constitute certain of them and which we call the angular drift. This is surely a most important and critical conclusion, and puts the problem entirely in a different position. It, in fact, limits the demands upon the force whatever it was that distributed the drift very much.

Are we obliged, or in fact, are we justified in invoking a great Ice Age with its portentous ice-sheets (whose dynamical powers cannot be equated with the demands made upon them by the glacialists) in order first to account for the striæ on the polished rocks and on the boulders, and secondly, for the manufacture of the angular drift. To my mind the questions only need to be asked to answer themselves. I have examined both problems in chapter xiv. in the work that follows (see vol. ii., pp. 321-338), and have shown that the striæ can be and ought to be assigned to an entirely different agent than ice if we are to follow inductive methods. They are, in fact, the necessary result of the distribution of the drift itself, however caused, and of the friction of its contents in such distribution against each other and against the rock surfaces over which they passed, and in most cases that have been quoted, are perfectly inconsistent with any ice action whatever.

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[1] I ought to have qualified what I say in the text in regard to the polished surfaces by here adding the words "or younger," since I hold that most of those in Greenland and Norway are contemporary with the recent upheavel of the land and were largely caused by a shingle-laden sea on a rising coast.

In regard to the angular stones of all sizes, from the angular gravel to the great erratics, I have argued that they are the witnesses of the only force capable of breaking the hardest and most crystalline rocks in this fashion, namely, violent impact at a time of great disturbance in the earth's crust, nor can I see by what process ice could in any way have made them. For my evidence and proofs I must direct my readers to the following work (vol. ii., pp. 338-343).

Let us now turn from the contents of the drift beds to their mode of distribution, internal arrangement, and external contour.

In all these respects the glacialists seem to me to have taken the greatest possible liberty with the evidence when comparing the drift beds with the normal or abnormal deposits made by ice as known to us.

The largest masses of ice known to us in the form of glaciers deposit the results of their denuding activity in the form of great irregular heaps and mounds of heterogeneous rubbish, in which clay and sand and stones are mingled in confusion and which are called moraines. The drift beds over by far the largest portion of the ground where they occur are, on the contrary, laid down in a continuous mantle covering hill and valley, where nothing resembling moraines is to be found at all (see vol. ii., p. 370). Where there are collections of irregular mounds, which have been classed as moraines by eager inquirers in America and in Germany, their internal structure is so different to that of moraine-stuff that it seems to me that if they were the product of an ice-sheet the ice must have been some form of transcendental ice. I have also discussed at considerable length other forms of pseudo-moraines which have been entirely misread by enthusiastic glacialists (see the following work, vol. ii., pp. 421-439).

Secondly, moraines as we know them form the outer limit of the glacier's depositing activity and mark the extreme extent of the ice flow. In these extraordinary so-called terminal moraines of the Ice Age we have a wide fringe beyond and outside the ramparts and mounds which has been a puzzle and a difficulty for the glacial men that has not been as yet rationally solved by them (see vol. ii., pp. 422-423).

Again, in more than one large district occupied by drifts we find insular areas entirely free from any traces of it, which seems impossible if the depositing agent was an ice-sheet (see *Glacial*

Nightmare, pp. 764 and 883, and in the following pages, vol. ii., pp. 370, 371). Again, in order to bring the great mantles of drift into line with supposed and suggested ice deposits, it has been the fashion for the glacialists to draw arbitrary lines, marking quite arbitrary frontiers as limits of these beds. As I have shown at considerable length this is quite inexcusable and, in fact, absurd. So far as we can judge these lines mark only imaginary limits, the fact being that the drift beds continuously change in character as we travel south both in Europe and America, and change gradually and without any breaches from stiff boulder clays with great boulders to gravels and sands and, lastly, to loess, the whole forming one mighty continuous deposit marked by the same débris of life, and marked also by a general sorting of materials as we travel southwards, of which gravity is the great ruling factor, and which therefore differ as much as anything well can from all the true glacier deposits known to us in which there is no such sorting. To this issue I have devoted a whole chapter of the following work, namely, chapter xii.

Again, this sorting is not confined to a general change from heavy materials to light ones as we travel south but extends to the local beds themselves, where we continually find the clays sorted from the sands and gravels, which occur again in definite and separate beds although in no regular order. Such conditions are quite inconsistent, as I have argued and I hope proved, with the operations of ice (see vol. ii., pp. 316-326). The absence of any order in the beds of sands, gravels and clays over more than a very small area is another powerful argument against these beds having resulted from the operation of the measured tread of a succession of definite phases in an Ice Age.

Next, the occurrence of masses and pockets of clay with their internal structure undisturbed enclosed in continuous beds of sand, and of stratified sand also internally undisturbed in beds of clay, and the similar occurrence of contorted beds lying on others with their laminations quite horizontal, seem quite inconsistent with the action of ice, unless it be that transcendental ice which is supposed to be able to dig out lakes and fiords from crystalline rocks and at the same time to pass over delicately laminated clays and sands without in any way disturbing them (see *Glacial Nightmare*, p. 697, and vol. ii., pp. 329-330, of this work).

The occurrence of convergent and divergent streams of boulders

in many areas covered by drift, and also of a medley of boulders from many sources not in the direct line of march of any possible ice-sheet, seems quite unaccountable if ice was their porter, as does the sorting of such stones according to their size as we travel from their mother beds to the outer limits of their distribution (see *Glacial Nightmare*, pp. 765-772, and vol. ii., pp. 333-336, of this work).

The presence in many cases of stones standing on end in the clays or symmetrically arranged according to their longer axes (see vol. ii., pp. 336-337), and the existence of laminated and stratified beds in the drifts, are also critical objections to their having been laid down by ice.

Again, there has never been any attempt made by the glacial champions to explain how ice could, under any conditions, transport under its mass vast quantities of soft and disintegrated materials, in some cases several hundreds of feet thick. An appeal has certainly been made to what are called ground moraines, but this is a kind of moraine virtually unknown in living glaciers, and its *modus operandi* I have never been able to understand. I have discussed the question in my *Glacial Nightmare* (pp. 684-697) and in the following work (vol. i., pp. 407-430, and vol. ii., pp. 296-302 and 366-369).

So difficult has the problem become of explaining the porterage of these vast deposits of loose materials of vast thickness by means of these most hypothetical and imaginary ground moraines, that, in America, the latter have been largely given up in favour of a new transcendental appeal, namely, the so-called theory of englacial drift, which I have similarly discussed and discarded as impossible to understand or to correlate with the actual ways of nature.

Another difficulty which has hitherto baffled all the efforts of the ice men to explain is the detachment and subsequent conveyance of large cakes and tabular sheets of solid strata, several hundred yards long, which are found imbedded in the drifts (see *Glacial Nightmare*, pp. 700, 701, and vol. ii., pp. 356-362, of this work).

The occurrence of the drift in various kinds of abnormally shaped ramparts and mounds known as äsar, eskers, kames, drumlins, etc., for the most part formed of rolled materials, largely stratified, with thin layers often arranged concentrically, and in the case of the äsar having marine shells in their upper

layers and larger erratics scattered over their surface, has exercised the almost incredible ingenuity of the glacialists in involving fantastic causes to explain them.

They have summoned sub-glacial and infra-glacial rivers and all the machinery which might occur to children in the nursery to solve problems of which the really incommensurable factor is their own hypothesis of an Ice Age. I have examined these mounds of different kinds at great length and the various theories which have been forthcoming to explain them, because they form not only most important elements with which any final theory professing to explain the drift must deal, but because they afford excellent examples of the unlicensed libertinism in speculation which has been thought consistent with the gravity of scientific discussion in our unfortunate science. They will be found discussed in the *Glacial Nightmare* (pp. 786-796) and the following work (vol. ii., pp. 373-422).

I have now finished my imperfect survey of the contents of the following volumes, in which I claim to have shown that the drift beds are, in their structure and arrangement, quite outside the capacity of ice to compass. These views I have urged for many years and with increasing conviction of their soundness. They have been sustained and supported by many journeys among the drift deposits of the old world in many climates, and by a very extensive reading of the literature, in English, German, French and Italian memoirs. Whatever presumption there may seem to be against them, involving as they do a revolt against a widely accepted theory, must be qualified by the fact that during the last twenty-five years there has been a gradual and continuous withdrawal from the position maintained by the more extreme glacialists, and a continuous moderating of their extravagance in the direction I have fought for. Many who were once very positive are now very doubtful, and the glacial theory has now a very different form among the young men who still adhere to it to what it had when Croll was writing his *Climate and Time*, and Prof. Geikie was bringing out the first edition of his *Great Ice Age*. I am content to confidently repeat Mr. Gladstone's most famous epigram, and to say, " Time is on our side ".

The issue is not complete, however, so long as it involves a merely negative polemic against the efficiency of ice action as an ultimate explanation of the drift phenomena. It involves, and it

should involve, a more positive complement, namely, the suggestion and working out of a more rational and logical theory which will cover a greater number of facts and be more completely in unison with scientific methods and inductive necessities.

This alternative, I have always maintained, exists, and was universally accepted before the world was dazzled by the factitious glamour of Agassiz's rhetoric, and especially by the escape it seemed to offer to the fanatical adherents of the theory of uniformity as expounded by the disciples of Lyell, more especially Ramsay and Jukes. Their real inspiration has been the fervent hope embodied in the words with which Sir R. Ball concludes his ill-fated book on the *Glacial Age.* "The appeal to ice," he says, "removed the glacial period from the position of a 'catastrophic' phenomenon. It placed the ice-sheet as an implement at the disposal of the geological uniformitarian." That was the real basis and inspiration of the new theory. That was what gave it its hold upon the geologists of a generation ago. They did not stay to ask whether in their zeal in favour (not of a real doctrine of uniformity, but of a bastard one) they were not giving themselves up to a scholastic figment and appealing to a fictitious and imaginary instrument in order to save them from what they then deemed the most pestilent of heresies, namely, catastrophism in any form.

Now that the glacial theory has utterly failed to justify itself in every way, it is surely time to ask whether those geologists who can distinguish science from sciolism and are wedded to induction wherever it may lead them are not going to try a new departure. I would ask them if they are quite content to go on snatching at quite impossible straws while struggling with a very whirlpool of difficulties created for them by their scholastic formulae, and whether they should not entirely reconsider their position and re-examine an alternative theory which satisfied the fathers of their science. These old masters were better equipped than most modern geologists. They were fine scholars and trained mathematicians and physicists as well as geologists, and, in regard to this particular issue, they had before them ample and abounding materials for a sound judgment. Murchison and Hopkins, Sedgwick and Philips, Conybeare and Von Buch, etc., etc. Towards their views Prestwich and Lartet, who are gone from us, as well as some distinguished living geologists I·

will not name, have been steering lately in various ways and with certain limitations.

The phenomena of the drift deposits were very well known to these old writers whose devotion to the genuine theory of uniformity was as great as that of any person now living, but who measured the necessary intensity of causes in past times, not by the puny experiences of man's short pilgrimage on this earth, but by the corresponding necessities of ascertained effects and results. To them the word catastrophe was clearly written in hundreds of geological sections as a recognised agency in the development of the earth. They did not, any more than some of us who reverence their teaching, believe in an aimless, lawless, arbitrary catastrophism, but in the perfectly solid doctrine that catastrophies form a real element in the regulated machinery of nature.

These old men assigned partially to subterranean movements and partially to the operations of water what their descendants have assigned to ice.

They had this great advantage, *in limine*, in that the forces they appealed to were not only explainable, but efficient. They were demonstrably capable of securing the results which they attributed to them. Our new philosophers have a very different touch-stone. Their appeal is to what is both inexplainable and inefficient.

The elaborate analysis of the phenomena which are supposed to witness a glacial age, which occupies the following pages, ought to suffice until some attempt of some kind is forthcoming to justify a theory so honeycombed with difficulties as the glacial theory is. Inasmuch, however, as it still has the sanction of many influential names, and is still the creed of many others whose science is merely the diluted wisdom or the wonder they derive from the prophets of the hour, I have thought it better to supplement it by a general survey of the whole geographical area where the drift phenomena occur in order to examine and criticise the more or less local phenomena which they present. The larger part of this local survey must be postponed to a succeeding volume. In the work as now published there is one chapter, however, devoted to this local survey, namely, the last one (chapter xvii.). It deals with the evidence forthcoming from the Arctic regions.

If there be an area where the evidence for the glacial period ought to be abounding and conclusive, it is surely in the very

focus of the earth's most severe climate. I would ask those students whose minds are still free and flexible, and who are not committed to any extravagant theories about ice, to read this chapter and to study the facts contained in it as an excellent touchstone of the whole theory. I claim to have shown in it that the Arctic lands, instead of being a low, flat district with a huge ice-sheet upon one portion of it (namely, in Greenland), as Croll and others have urged, are very largely elevated plateaux, and that the present great cold of the Arctic regions is due primarily and chiefly to the fact of so much of them being high lands and not low lands, and to the consequent accumulation of a vast reservoir of cold untouched by the vicissitudes of the seasons (see vol. ii., pp. 440-455). If the land now existing in the highest latitudes were low instead of high as now, it is clear that there would be little if any permanent ice in the Arctic regions, as there is no permanent ice or snow in the flat islands to the north of Greenland and in the low tundras of Siberia.

Secondly, I have tried to show from the evidence of raised beaches, etc., etc., that the last movement of the land in the Arctic regions has been one of upheaval which is still in progress, and that consequently the climate of those regions, instead of being less severe now than formerly, is more so, and is becoming daily more severe (vol. ii., pp. 455-463). Of the growth of this severity I have given very considerable proofs apart from its *a priori* probability (vol. ii., pp. 463-487).

The conclusion from this analysis I will state in words used in the chapter I am referring to : " It would seem, in fact, that until the current geological period there never was such an elevated land in the North Polar region as we now find there, nor anything like the rigorous climate that now prevails there. If this be so, it means that the Arctic lands furnish us with no *a priori* evidence that a so-called glacial period ever existed there at all, unless we can call the present and current period a glacial one " (vol. ii., p. 477).

Having established this conclusion, I then proceed to test the *a priori* position by the positive evidence forthcoming from the polar area and to show that there are, in fact, no traces of a former Ice Age in Iceland or Greenland and the other Arctic lands where all the glacial phenomena are recent (vol. ii., pp. 477-487). The biological evidence, both living and recently extinct, seems to be

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conclusive that these northern lands, instead of being an area recovering from glacial conditions, is one where formerly more or less temperate conditions prevailed, accompanied by a much richer and more abundant fauna and flora which have become impoverished and been thinned out by the increasingly severe conditions of the climate, and that the only glacial period supported by any evidence in the Arctic regions is the present one (vol. ii., pp. 487-498).

In a subsequent volume I propose to extend this analysis of local conditions to all the other regions where the glacial period is supposed to have left its mark, and to prove that everywhere the testimony is the same, namely, that the phenomena when critically examined entirely refuse to be equated with that theory. I then propose to show that the effects which the glacialists have attributed to ice, and which ice has been proved incapable of compassing, were due to water whose capacity to compass them cannot be gainsaid, a view I have already elaborated in previous works.

Meanwhile, I venture to urge that the case here made out is too consistent and powerful and overwhelming to be answered by mere fatuous silence. Old men who have written foolish things and said unwise ones do not easily retract or explain. They are welcome to the momentary triumph involved in the bewildered acquiescence of their fading contemporaries and quondam scholars. This kind of reputation is easily earned if they are content to see the great besom of Time sweep their theories and their works into oblivion. That is not an exhilarating prospect however. To all of us who have learnt how much of man's most cherished creed is vanity, it must be clear that the large part of what we do or write or say is necessarily ephemeral. We could only secure ourselves against errors by being omniscient, and others will certainly follow us who will straighten our crooked ways; but most of us would like to feel that in the future some men may turn to something that we have done and allow that there was permanent light and leading in it. Among the younger men, as I know well, there has been a gradual and recently a more rapid revolt against the extravagances of a previous generation of writers on the glacial theory, a clinging closer to scientific proof and induction, and a repudiation of metaphysical and transcendental postulates and dreams. It is with them I would travel along the

attractive road that leads towards the light, whatever prejudices and predilections have meanwhile to be sacrificed.

In writing the following work I have been dependent, as every man must be, very largely upon the work of others. I have spent much of my time in examining the drift beds in many countries, and more still in trying to sift the vast literature of the subject, and I can frankly say that I have not willingly or wilfully shirked a single difficulty, and have tried further to do full justice both to those with whose conclusions I agree and those with whom I disagree. ' I do not know from which section I have learnt the most. I have quoted their own words in every case where I could, and I hope I have faithfully given credit to each for his own discoveries. Any other method I deem to be dishonest. Scientific men who do not quote those from whom they have taken their facts or opinions are very nearly charlatans. If there be puzzles and difficulties and pitfalls which I have not noticed and tried to elucidate in either of the two works on the Glacial Nightmare which I have written, or if I have made mistakes of fact or inference, it will be a great delight to me to confess them and correct them in my subsequent volume if any critic will send them to me. Mistakes I know must abound in a work dealing with so many tangled issues and with such a multitude of facts, and I apologise for them.

One thing more I would say. It is not easy to conduct a polemic against a dominant school of scientific thought which has shown itself at times arrogant, supercilious and wanting in courtesy, without using some of the energetic adjectives and adverbs which their attitude has inspired, and which my inherited gout has made rather prolific. These adjectives and adverbs have been directed, I hope, entirely against methods and modes of scientific argument, which I deem illegitimate as logic and questionable from the point of view of the ethics of science. Science likes her children to be frank, ingenuous, modest and painstaking. She respects those most who most openly recall their errors and who give themselves no airs when they bow their heads at the temple of wisdom where we are all scholars. She has the longest and most lasting contempt for those who, because of pique or assumed prestige, fancy that the authority she sanctions can be found anywhere except in sound induction, and who allow the great crowd to continue to be misled by mistakes they have made

and which have been exposed, and by misleading and sophistical rhetoric.

Clerical and other mistakes I have corrected in a table. Its length is due partially to my bad eyesight and partially to the fact that the book has had to be written with many distractions and at times with the burden of indifferent health. These infirmities must account also for the limping sentences which occur betimes, and for the irregular order in which the argument has been sometimes presented.

I have to thank my friend Culverwell for looking over chapter i., which deals with a subject he has greatly illuminated, and another very old and dear friend, McKenny Hughes, who has looked over two or three of the chapters in reference to pseudo-glacial phenomena and supposed former ice periods. I also return my warm thanks to my father Anchises, Dr. Henry Woodward, who has allowed me a very generous use of the pages of his quite unique *Geological Magazine* to air the views here developed. I must also name the late Mr. John Murray, the famous publisher. He wrote a book once, the anonymous authorship of which was preserved for nearly half a century, until it was, I think, first disclosed to myself. It was entitled *Scepticism in Geology.* It is full of wisdom, and I have profited from it. My son Rupert and my friend J. D. Hardinge-Tyler, both of them worthy products of that good school for training English judgment and sound sense, New College, Oxford, have used their sharp eyes and wits in searching out the motes and beams that have escaped my poorer vision in these pages. I thank them as I also do my patient friends, the printer and his readers, anonymous benefactors whom we are apt to overlook, and who have been, no doubt, sorely tried by my writing. I must also thank the hundreds of authors I have read, and who have given me much consequent delight and pleasure and instruction. Last, not least, my gratitude is due to those of mine own household who have taken care that no glacial breezes or icy streams have qualified the perennial sunshine of my home. In that haven I am, however, obliged to daily remember what the Chinaman so well said, namely, that "the conjuror cannot take in the man or woman who plays the banjo for him".

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CORRECTIONS.

Page 11 line 5.   For "rest" read "pass".

„ 15 „ 3.   For "28″" read "58″".

„ 38 „ 39.   Erase the first comma, and in line 40 erase "it".

„ 40 „ 28.   For "present" read "former".

„ 44 „ 5.   Erase "makes the earth spin" and substitute "the earth engenders in spinning".

„ 50 „ 29.   For "Peruter" read "Pernter".

„ 64 „ 23.   For "sun" read "river".

„ 70 lines 27 and 28.   For "correlevant with" read "correlative to".

„ 108 line 34.   After Thomson read ("now Lord Kelvin").

„ 112.   Insert inverted commas before "Neumayr" in line 9 and after "Siberia" in line 18.

„ 127 line 26.   Insert pp. 52-59 after "age".

„ 127 „ 34.   Insert "interference" after "which".

„ 139 „ 20.   For "Sleimnann" read "Steinmann".

„ 152.   Transfer (p. 370) from line 25 to the end of the paragraph.

„ 156 line 5.   Substitute a full stop for the note of interrogation.

„ 164 „ 14.   For "ocks" read "rocks".

„ 175 „ 36.   For "include" read "included".

„ 177 „ 40.   For "Dusseldorf" read "Düsseldorf".

„ 210 „ 33.   For "have" read "has".

„ 222 „ 36.   For "French Swiss" read "Romansch Swiss".

„ 228 „ 19.   Insert "in" after "so".

„ 234 „ 3.   For "cadet questio" read "cadit quæstio".

„ 241 „ 10.   For "Rock" read "Roch".

„ 241 lines 33 and 34.   I ought to have said these dunes advanced at this rate from 1666 to 1722.

„ 282 line 28.   For "full" read "empty".

„ 320 „ 6.   For "Virchof" read "Virchow".

„ 323.   In the headline put "Lyell" instead of "Murchison".

„ 325 line 23, and 337 line 32.   For "Lurley" read "Lorelei".

„ 337 „ 27.   For "ladened" read "laden".

„ 338 „ 9.   For "Goschenen" read "Göschenen".

„ 362 „ 20.   For "Annecy" read "Annécy".

„ 369 „ 23.   For "appeared" read "appear".

„ 371 lines 3 and 4.   This is not very clear.   What is meant is that the rocky floors of the fiords rise abruptly at the fiords' outlet into the sea.

Page 380 lines 28, etc., and 405 line 40, -406 line 3.   Mr. Hardinge-Tyler has pointed out to me that these paragraphs as they stand are ambiguous.   What I wish to insist upon is that dry snow cannot be converted into ice.   It must be damp snow.   The localities where snow remains quite dry under present conditions of climate must be very limited.   As my friend points out even on the highest Alps there are warm winds, while the sun is sufficient to melt off the snow which rests on dark rocks even at great heights.   It seems plain, in fact, from the borings made on the summit of Mont Blanc at Mr. Janson's observatory, to which attention has been called by Whymper, that snow is converted into ice near the summit of that mountain, as it is in the ice cliffs on the Jungfrau, and I may add, in Greenland, all which cases are much above the snow-line.   On the other hand, there are virtually no glaciers on the northern slopes of Thibet and on the western slopes of the higher Andes where the snow remains dry snow.   What is needed to convert snow into ice is moisture and pressure.   As schoolboys know very well, " frozen snow " or dry snow will not make snow-balls; *a fortiori*, it will not make iceballs.

,,   382 line 19.   For " tail " read " snout ".

,,   384   ,,   27.   For " Mosley " read " Moseley ".

,,   405   ,,   39.   After " rain " insert " or warm winds ".

,,   407   ,,   21.   This is not quite correct.   The stones do not move down to the crevasses, but the crevasses open at or near where the stones happen to be when the ice reaches broken ground.

,,   408   ,,   24.   I ought to have added after " ground " " or going round corners ".

,,   410   ,,   10.   For " geologist " read " glacial geologist ".

,,   426   ,,   22.   For " tourbeuxe " read " tourbeux ".

,,   433   ,,   16.   For " *fur* " read " *für* ".

,,   478   ,,   38.   For " Suvretta " read " Silvretta ".

,,   481   ,,   32.   For " depth " read " level ".

,,   499   ,,   10.   For " has " read " have ".

,,   532 lines 21 and 22.   For " Sea " read " See ".

CHAPTER I.

THE ASTRONOMICAL THEORY OF AN ICE AGE AND ITS LATEST CHAMPION, SIR ROBERT BALL, LL.D., F.R.S., ETC., ETC.

"The causes of notable geological changes must be other than the relative position of the sun and earth under their present laws of motion" (Meech, *Smithsonian Trans.*, ix., p. 41).

WHEN under the magnetic influence of Agassiz's persuasive rhetoric, the world of science accepted the former existence of a great ice age as a reality and not a mere fantastic creation of the imagination, and serious men in serious books agreed that the greater part of the earth was in recent geological times swathed and smothered in ice, it was natural that they should go a step farther. They, in fact, proceeded to urge that such an ice age was no miraculous and unique display of nature's versatility, no portentous break in its continuous annals, but a regular and recurring incident, the inevitable result of the recurrence of certain astronomical conditions, and capable therefore of proof, not merely on inductive grounds, but deductively from the laws that govern the movements of the earth in regard to the other heavenly bodies.

This conclusion was naturally most attractive to men who in their scientific hearts reverence that explanation most which has the most automatic appearance. Agassiz's facile words and illustrations had captured their judgment in regard to the former presence of ice almost everywhere. What they now hungered and thirsted for, was some explanation of such an ice age which should be based on astronomical grounds, and should be therefore safely buttressed by mathematical proofs, and one whose recurrence at definite intervals should be shown to be also consistent with the recurring chimes that mark the march of time, and a mere incident in a perfectly consistent story.

Appeals were made to the astronomers to come to the rescue of this new geological gospel. The astronomers were powerless however. Occasionally one could be found who gave some very mild and coy countenance to the notion that the stupendous postulate which had been propounded by Schimper and proclaimed by Agassiz might perhaps have some slight justification in the varying eccentricity of the earth's orbit, and the varying obliquity of the ecliptic, but they did not venture beyond coy suggestion. Those who examined the problem thoroughly and faced its real conditions were positive and definite in their conclusion about it.

This conclusion is formulated clearly and unmistakably in the sentence at the head of this chapter, which is quoted from a distinguished member of their body, Mr. Meech, and which I will here repeat. "The causes of notable geological changes must be other than the relative position of the sun and earth under their present laws of motion."

This conclusion was for some time accepted by the geologists with resignation and humility as a *res judicata*, and by no one more definitely than by Croll, who may be looked upon as the most acute and extravagant champion of the glacial nightmare on physical grounds, and by his henchman, Professor James Geikie, the most distinguished geological champion of the same views whom we have produced in these realms. They both repudiated a *purely* astronomical cause as quite inadequate and incompetent to account for an ice age (see *Glacial Nightmare*, pp. 354-361).

When I was writing my *Glacial Nightmare*, in which I subjected the astronomical theory of an ice age to a minute examination and criticism (*op. cit.*, pp. 354-376), I virtually treated that theory therefore as dead and obsolete. It is never safe, however, to act the part of an undertaker to any scientific hypothesis. However illogical, however at issue it may be with the facts and the deductions of mathematics or sound induction, it is pretty sure to be resuscitated somewhere, so long as men crave continually for some indisputable basis for their prejudices, and no human prejudice is more stubborn than the creed sometimes imposed upon a dominant scientific school by some masterful teacher.

Thus while the book I refer to was being written, the

astronomical explanation of an ice age in its most extreme shape was being again propounded, and it was presently urged from no less a platform than Adams' Chair at Cambridge.

There can be no doubt that the appearance of Sir R. Ball's work on *The Cause of an Ice Age* in 1891 was an epoch in this discussion. Sir R. Ball was then Royal Astronomer for Ireland. He now fills one of the most famous astronomical chairs in Europe, and he is the author of at least one mathematical classic.

It was assuredly a serious responsibility for him to throw over the long list of distinguished astronomers and physicists who had emphatically discarded an astronomical cause of an ice age as inefficient, and to put before a great mass of students of geology, few of whom could verify his methods or his calculations, a scheme which, if untenable, must necessarily do infinite mischief to science, and lead astray those who most want help in these discussions.

Sir Robert Ball expressed no hesitation or doubt on the matter, and he took up the most uncompromising position in regard to it. He tells us in his work that it is his purpose and aim, not merely to explain changes of climate by astronomical causes, but to show that the astronomical theory itself is sufficient to account for an ice age without having recourse to other causes. He urges that the additional conditions thought necessary by Croll, by J. Geikie, and by Wallace, for the explanation of an ice age, were in fact not necessary. " Once the true significance of the astronomical facts has been realised," he says, " it is apparent that, leaving the other agents much as they are at present, the alteration in the range of temperature will be amply sufficient for even extreme changes in climate " (*op. cit.,* p. 133). " These are brave words." The influential source of the pronouncement and its crucial importance make it incumbent upon me to examine Sir R. Ball's position in some detail, and I must be pardoned for discussing some elementary matters, since the fallacy involved is an elementary one.

If the earth's orbit were circular, as the ancients thought it was, it would move through the same space daily and receive precisely the same amount of heat per day during the whole year. Kepler proved for all time, however, that

the path of every planet moving round the sun is an ellipse, of which the sun occupies one of the two foci.

When Newton had discovered the law of gravitation, it was speedily seen that the attractions of the planets upon each other must necessarily affect the form of the ellipse in which each planét moves, and inasmuch as the positions of the planets towards each other is constantly varying, it follows also that there is a constant variation in the forms of the planetary orbits.

The calculation of the exact figure of the earth's orbit at any moment (the result as it is of a complicated series of planetary and other attractions) is a problem of some difficulty, but it has been faced with complete success, and the calculation has been made for very long periods by Leverrier, Stone, Croll and others.

The variation mainly consists in the degree in which the elliptical orbit approximates to, or departs from, a circle in shape, or, ih other words, in the variation of the relative proportions of its longest and its shortest axis. The variation chiefly affects the shortest axis, the longest one being nearly constant.

Lagrange and Laplace showed by a superb piece of mathematical analysis that while there is a continual variation in the amount of eccentricity of the planetary orbits, they do not vary *indefinitely*. While the minimum may theoretically reach zero, there is a distinct maximum beyond which the variation cannot extend. This limitation secures the stability of the solar system.

Lagrange, for whom the data based on the attractions of Uranus and Neptune were not available, made the maximum in question 0·07641. Leverrier, whose tables have been followed by Croll and others, and who did not take into account the action of Neptune, nor terms of the third order, made it 0·077747. Stockwell, whose figures I adopt, taking into account the action of Neptune and improved values for the masses of the other planets, made it 0·0693888.[1] Between this absolute maximum and zero almost every variation is possible, and has occurred.

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[1] Sir R. Ball, I do not know on what basis or what grounds, puts the maximum eccentricity at 0·071, which is very near Stockwell's figure.

The variation is not continuous, however, but very irregular, and there are a series of successive enhancements and depressions between the two extremes. These lesser maxima and minima, if we may so speak, are themselves very unequal in extent, and are attained after very unequal periods. Stockwell calculated the degree of eccentricity attained at intervals of 10,000 years for 2,000,000 years before his time.

McFarland recalculated the tables from Stockwell's data, and greatly extended them, giving us the eccentricities for intervals of 10,000 years, from 3,260,000 years before the year 1850 to 1,260,000 after 1850 (*Amer. Journ. of Science*, 1880, pp. 107-111). During the whole of this vast period the highest eccentricity reached was about 840,000 B.C., when it was ·0649. The next maximum was about 570,000 B.C., when it reached ·0535. In 470,000 B.C. it reached ·0437. During the last 400,000 years the three highest maxima reached by the eccentricity have been in 300,000, 210,000 and 100,000 B.C., when they were ·0373, ·0471 and ·0408 respectively. From the last of these culminating points of ·0408, 100,000 ago, the eccentricity reached a minimum of ·0110 in 40,000 and 50,000 B.C., whence it rose to a slight maximum of ·0195 in 10,000 B.C. It then gradually dropped to its present figure, ·0168, and will continue to drop until about 24,000 A.D., when it will reach a minimum not touched for 500,000 years before, and be virtually at zero. Thence it will rise to a small culmination, ·0145, about 60,000 A.D., then sink to ·0110 in 90,000 A.D., whence it will rise to ·0288 in 150,000 A.D., and so on. Those who wish for greater details will find them in the tables published by Stockwell and McFarland in the *American Journal of Science* for 1868 and 1880.

These figures are undisputed. Their importance for us in our present quest is not however their direct testimony, but their indirect testimony, namely, what changes they imply in the earth's climate as the result of a varying eccentricity.

It was long ago shown by Herschel and others, and indeed it follows from a simple mathematical calculation, that the earth is actually nearest to the sun throughout the year when the eccentricity of its orbit is greatest, and most dis-

tant from it when its orbit is most circular. Inasmuch as the amount of heat it receives from the sun is proportional to its mean distance from that source of heat, it follows that the quantity of heat received by the earth from the sun in a year is least when its orbit is most circular, and greatest when it is most eccentric, and that the more eccentric we make its orbit the more heat will reach the earth from the sun in a twelvemonth.

Inasmuch, again, as the earth's orbit has for many thousands of years been becoming gradually more circular, it follows that the mean heat it has received from the sun has been gradually diminishing, and inasmuch as that orbit is now very nearly at the most circular stage which it can possibly reach, it also follows that the total mean heat received annually by the earth is almost at its *minimum*. Hence, *if we limit ourselves to the element of eccentricity alone*, it is plain that whatever potency it may have had in altering the total amount of heat received by the earth has been exercised during the last 10,000 years in the direction of creating a glacial period rather than in undoing it, and that never in all time have the conditions been much more favourable than now for the existence of such a period. As a matter of fact, however, the variation is really immaterial.

The difference between the maximum and minimum of total heat which can be thus received is so slight that it is hardly worth considering at all. I have given some illustrations of its slight amount in my former work (*Glacial Nightmare*, pp. 355, 356), and will here quote only one note from Meech. Adopting Leverrier's figures, he concludes that the greatest variation which has taken place in the last 100,000 years, or which can occur in the next 100,000 years from this cause alone, namely, that between the limits of ·0473 and ·0168, only amounts to four or five hours of sunshine a year; and he adds that if we take the superior and ultimate limit given by Leverrier, namely, ·0777, which he says was reached 850,000 years ago, the annual intensity of sunshine in a year only exceeded that of the present time by thirteen hours of sunshine in a year. On the other hand, the inferior limit of eccentricity which is very near to zero indicates only four minutes of average sunshine in a year less than the present annual

amount.[1] " Between these two extreme limits," says Meech, " all annual variations of the solar intensity, whether past or future, must be included even from the primitive antediluvian era, when the sun was placed in his present relation to the earth " (*Smiths. Trans.*, ix., p. 36).

It is thus clear that the total amount of heat received by the earth each year is virtually the same under all possible conditions of eccentricity. This conclusion is quite conceded by Sir R. Ball (*A Cause of an Ice Age*, *vide* pp. 79, 80 and 85). He says, in fact, that the annual supply of solar heat to the earth has been practically invariable.

It is also plain that the distribution of this heat over the different portions of the year is nevertheless subject to considerable variation in correspondence with the varying eccentricity. This follows from some simple considerations. The amount of heat received by the earth (as a whole) from the sun varies momentarily. It is dependent on the distance it is at any time from that luminary, and is, in fact, in the inverse proportion of the square of that distance. Inasmuch as the sun occupies one of the foci of the earth's orbit, it is nearer to it in one half of its journey than in the other. If the earth always travelled at the same rate therefore, a large proportion of the solar heat which it receives would be concentrated into that part of its journey when it is nearest the sun, and a small proportion would be concentrated in the remaining part, causing two highly contrasted earth-seasons (*i.e.*, seasons affecting the world as a whole) annually. This result is greatly modified and affected, however, by another one.

Kepler showed by a marvellous piece of combined observation and induction that every planet which moves round the sun in an elliptical orbit must move so that the line joining it to the sun always travels over equal areas in equal times. The shape of the areas will momentarily differ, but their superficial contents will always be the same for the same time. This is known as Kepler's second law. From it, of course, it follows that the rate of movement of the earth

[1] If Stockwell's numbers are adopted, these figures will have to be reduced. Becker says that with zero eccentricity the earth would receive ·00014 less heat per annum than it does now.

supply of heat is the same, or virtually the same, while the rate of radiation is treated as Sir R. Ball treats it as invariable, the fact of concentrating one half of it into fewer days and distributing the other half over a greater number of days has no effect upon the thermal work done by the sun's rays as a whole during the year. Whether the same amount of heat is applied quickly or slowly is indifferent in regard to the amount of work done. The total heat supplied by the solar furnace remains precisely the same in actual degrees or thermal units and in actual efficiency, whether converged upon a short hot period or distributed over a cool long one, and it does precisely the same amount of dynamical work. It is true that in a short hot period it works more quickly and in a long cool one more slowly, but the final result in undoing at one period the opposite work of the other is precisely the same.

In order to create an ice age in the way suggested the cold of one period of the year must gain upon and accumulate over the heat of the other, and we can only secure this if the rate of radiation is constant, by reducing the total number of heat-units received during both periods taken together. To increase the heat of the stove is to mitigate the cold faster, and in doing the work faster we can of course devote fewer days, that is, a shorter period to it. Thus, however great the disparity in the length of the earth-seasons, so long as precisely the same amount of heat is distributed over both of them together, and the rate of radiation is constant, so long will a mere alteration in that disparity fail to affect the mean annual temperature of the earth, and therefore fail to induce an age marked by the accumulation and storage of cold, *i.e.*, an ice age. *A fortiori* must this be the case when the sum total of annual heat received is increased as it inevitably is with increased eccentricity.

This argument is unanswerable. Astronomers and others have been unanimous in their conclusions on the subject,[1] nor does Sir R. Ball himself contest the conclusiveness of the case they have presented. His contention, however, is that the change in eccentricity under discussion becomes a potent cause of an ice age *when it is combined with other causes.* Let us therefore move on.

[1] See *Glacial Nightmare*, pp. 857-861, and the opinions of Herschel, Arago, Meech, Croll and J. Geikie there quoted.

If the earth's axis were at right angles to its line of march, *i.e.*, to the plane of the ecliptic, there would be no strictly separated and defined seasons as at present, but a gradual change in the mean temperature all over the earth, from its hottest day, when it was nearest the sun, to its coldest, when farthest from it, and then *vice versâ*. The two hemispheres would have the same hot and cold periods at the same time ; each would have its hot period when the earth was nearest the sun, and its cold one when it was farthest from it ; all places on the same parallel of latitude, either north or south of the equator, would receive an equal quantity of light and heat in each twenty-four hours ; the portion of the globe lighted and heated by the sun would invariably extend (with a continuously diminishing quantity of light and heat) from the equator to the pole in both hemispheres ; the days and nights would always be twelve hours long ; and lastly, inasmuch as the sun's centre would always be immediately over the equator, the equator would always receive the greatest quantity of heat possible, while the two poles would be continually in a condition of most wintry cold.

The earth's axis, however, is not perpendicular to the plane of its orbit, but is inclined to it at a less angle than a right angle, and it follows that in one half of the earth's journey round the sun one hemisphere is continually facing that luminary and the other hemisphere is looking away from it, and in the second part of the journey this attitude is reversed. The result of this is that instead of the sun being continually over the equator, it seems to an observer of the heavens to travel into each hemisphere alternately, and it performs the double journey to and fro annually. The length of this apparent journey depends upon the angle at which the axis is inclined to the plane of the ecliptic. The nearer the angle is to a right angle the shorter the apparent journey of the sun. The two limits of its apparent journey north and south respectively are known as the solstices, from the sun's resting at each of them before it turns back again (*solis statio*). The two solstices correspond to the two parallels of latitude on the earth over which the sun rests at the limits of its journey, and which are known as the two tropics, the one in the northern hemisphere being named, from its

position in regard to the signs of the Zodiac in the days of the Greeks, the Tropic of Cancer, and that in the southern hemisphere the Tropic of Capricorn. In its apparent journey from the one tropic to the other and back again the sun seems to cross the equator twice annually, and it does actually rest immediately over the equator twice in every year. While it is in that position day and night are *everywhere* equal. Hence these two stages in its apparent motion are known as the equinoxes. At other times day and night are everywhere unequal. The days being longer than the nights in the hemisphere into which the sun is apparently travelling, while they are correspondingly shorter in the hemisphere it seems to be deserting. This preponderance of day over night, or the reverse, means an enhancement or curtailment of the sunshine to that extent, and means, of course, that the amount of light and heat derived from the sun is *pro tanto* increased or diminished accordingly. This constitutes the essential difference between summer and winter. It means further that the various conditions which would be present if the earth's axis were perpendicular to the ecliptic and to which I have referred are all reversed or altered.

It means that the two hemispheres do not have their warm and cold seasons at the same time. Each has its hot season not, as might be supposed, when it is nearest to the sun, but when it directly faces and looks at the sun, and its cold season when it looks away from him; so that the hot season of one hemisphere is the cold one of the other. Hence it follows that when there is a long summer in one hemisphere there is a long winter in the other, and *vice versâ*. Not only so: in consequence of the heat which reaches either hemisphere in its summer striking it directly, while its winter heat strikes it obliquely, the former is much more potent than the latter, or, in other words, each hemisphere receives a considerably greater quantity of heat in its summer than it does in its winter, whatever the respective length of those seasons may be.

Although differing in length the summers of the two hemispheres receive precisely the same quantity of heat, so do the two winters. The proportion between the heat received in summer and winter respectively must also be

precisely the same in either hemisphere so long as the obliquity of the earth's axis remains constant.

Inasmuch as the hot season in one hemisphere coincides with the cold one in the other, while the hot and cold seasons respectively vary in length in each hemisphere, it follows that when there is a short hot summer and a long cool winter in one hemisphere there is a short warm winter and a long cool summer in the other—that is to say, while there is a more sharply contrasted summer and winter climate in one hemisphere there is a closer approximation to a mean climate in the other.

To continue, however, the list of important terrestrial conditions of climate due to the obliquity of the orbit. Places on the same latitude in the two hemispheres do not, except at the equinoxes, have days and nights of the same length nor receive the same amount of heat and cold in the same twenty-four hours; the same latitudes in one hemisphere having an advantage or a disadvantage over those in the other respectively, according to the seasons in each. These differences alternate between the two hemispheres and cause a true summer and a true winter to alternate with them in each hemisphere, the summer being marked by the season in which the days are longer than the nights, and the winter by the reverse conditions. The limits of the two real and true seasons of summer and winter are marked by the two equinoxes, which are known as the vernal and the autumnal equinox respectively, while the commencement of the four so-called seasons of summer, winter, autumn and spring corresponds with the longest day and longest night, *i.e.*, the two solstices, and with the two equinoxes respectively.

As we have seen, the distance of the two tropics from the equator is a measure of the obliquity of the earth's axis, and a measure also of the distribution of the summer and winter climate in each hemisphere. If that angle were constant the distribution in question would be constant too. That angle, however, is not constant but is subject to variation. Hence it has been argued by some writers, whose astronomy is of the elementary kind, that in this variation we may possibly have a *vera causa* for an ice age.

Let us then shortly consider what effect such a variation

will have upon climate. The nearer the angle made by the earth's axis with the ecliptic is to a right angle, the nearer, of course, will the tropics be to the equator until, if the axis were actually perpendicular, they would completely coincide. On the contrary, the smaller the angle so made, the greater will be the distance between the tropics and the equatorial line until, if it were possible to make the earth's axis actually revolve in the plane of the ecliptic, " the tropics " would coincide with the two poles. The problem to be solved is, what effect would the movement of the tropics, either towards or away from the equator, have upon the climate of each hemisphere?

In the first place, then, this change would not increase or diminish by a tittle the total amount of heat received by the earth as a whole during a year, nor yet the amount of heat received by each hemisphere in a year, which is always one half of that received by the earth. The total amount of heat so received is dependent, as we have seen, entirely upon the amount of the eccentricity of the earth's orbit, and varies very little.

Secondly, any variation in the obliquity of the axis would not affect the momentary proportion of heat received by the earth as a whole in any part of its orbit; that proportion is also absolutely fixed by the distance of the earth from the sun, *i.e.*, by the amount of eccentricity of its orbit.

What would be affected would be the amount of heat received during each of the two seasons by each hemisphere and by the different zones in each hemisphere. It is perfectly plain that when the sun travels farther north and the tropic is accordingly shifted farther north, the summer temperature of the northern latitudes will be correspondingly increased, and so when the sun travels farther south the summer of the southern latitudes will receive an enhanced amount of solar heat. In a similar way the farther the sun goes north and enhances the heat received by the higher northern latitudes, the farther away will it travel from the southern ones, and *pro tanto* deprive them of warmth. This is plain enough; but it is only an example of the changes involved. These mean, in fact, that every latitude from the equator to the poles will more or less have its summer and winter climate

affected by a change in the obliquity of the earth's axis.
The calculation of the changes involved is an intricate but
not a very difficult problem. The chief effect of making the
earth's axis lie more nearly in the plane of the ecliptic (which
is called increasing its obliquity) would be to give hotter
summers and colder winters to the highest latitudes,and in this
way to increase the contrast between their seasons. This effect
would be slightly greater in enhancing their summer climate
at the expense of the tropics than in diminishing their winter
one. As Mr. Becker says : " Since an increase in the length
of the winter diminishes all heat rates or temperatures in the
same proportion, and since the rates are highest in the tropics,
the greatest decrease must also take place in the torrid belt "
(*Amer. Journ. of Science*, xlviii., p. 106). Instead, therefore, of
an increase in the obliquity of the earth's axis inducing a colder
climate in high latitudes and thus producing glacial conditions,
it would have the reverse effect. It would simply supply the
higher latitudes with more heat per annum than they have now.
This greater heat would be entirely in summer, and (always
granting the same rate of radiation) must more than dis-
sipate the effect of the relatively less increase in the cold
of winter, and instead of an accumulation of cold in the
higher latitudes there would be altogether a milder climate.
This has been overlooked by those who have claimed an in-
creased obliquity as a possible cause of a glacial age. If a
change in the inclination of the axis is to make the higher
latitudes permanently colder, it must be not by an increased,
but by a decreased obliquity. The polar climate would be
most nearly that of a glacial time if the axis were perpendicu-
lar to the orbit ; and it is in fact travelling in that direction
now, since the obliquity is now slowly becoming less instead
of greater. This was perfectly well seen and appreciated by
Croll, whose critical faculty was so acute ; and I have enlarged
upon it in my previous volumes, to which I must refer my
readers (see *Glacial Nightmare*, pp. 366-369, etc.).

Apart from the fact that no possible increase in the
obliquity of the axis would create glacial conditions, but the
reverse, there is a further difficulty, if one is needed, in that
the possible limits of variation as defined by Laplace are so
very narrow as to be almost inappreciable. Stockwell, who

has recalculated Laplace's original observations and arrived at virtually the same conclusions, has shown that the limits of possible variation in the angle of obliquity are from 24° 35′ 28″ to 21° 58′ 36″; whence it follows that the greatest and least possible declinations of the sun at the solstices can never differ from each other to a greater extent than 2° 37′ 22″; and as the present obliquity is very nearly the mean, the variation cannot be much more than one half of this latter amount in excess or diminution of the amount of obliquity at present prevailing. Taking this amount, Meech and Croll have calculated the greatest possible effect upon climate which an increased obliquity could produce. I have given the results at some length in my previous work (*op. cit.*, pp. 368, 369). The general conclusion is that when the obliquity was greatest the poles enjoyed 8·45 thermal days more heat than now per annum, or received twenty rays for every nineteen they receive now, and their climate resembled that of latitude 76°. This is the extreme possible limit. It is true that by a perverse process, in which he discards the reasoning and results of the greatest astronomers, and apparently ignores dynamics altogether, General Drayson, R.A., who professes to attack one of the most difficult of all analytical problems as a mere exercise in geometry, has come to a different conclusion. In a work published in 1878, and entitled *On the Cause, Date and Duration of the Last Glacial Epoch of Geology and the Probable Antiquity of Man*, and in subsequent works he professes to prove that the usual theory in regard to the precession of the equinoxes and nutation is unsound; and he would explain the phenomena so completely elucidated by Newton and Bradley, and traced by them to their efficient causes, as a proof that the motion of the pole is in a circle whose radius is 29° 25′ 47″, and whose centre is at a distance of 6° from the pole of the ecliptic. He further argues that this circle is described in a period of 31,840 years, so that at intervals of 15,920 years the obliquity of the ecliptic varies as much as 12°. The consequence of this would be, he argues, that about 13,700 B.C. Great Britain had during the winter an arctic climate, the sun in latitude 54° not being 1° above the horizon at the winter solstice, and during the summer a tropical climate. This is supposed

to have been the last glacial epoch, and such epochs are sup-
posed to have recurred every 31,840 years (see *Nature*, 1873, p.
301). Inasmuch as General Drayson disallows the existence
of precession and nutation, and combines the phenomena
supposed to attest them with those of a varying obliquity,
thus greatly enhancing the latter, he of course disallows the
reversal of the conditions on each hemisphere attributed to
precession, and with him the glacial period did not alternate
between the hemispheres, but existed in both contempo-
raneously. In his view, when the conditions were most
favourable every winter was a glacial period and every
summer an interglacial one, and his notion of a glacial
period was merely a period when the winters were much
colder and the summers much hotter than they are now. It
is only necessary here to say that no mathematician or
astronomer has given the slightest countenance to General
Drayson's paradox, while Croll (see *Glacial Nightmare*, pp.
366, 367) subjected it to an acute criticism of another kind, and
showed that even if it were tenable, it would not and could
not explain an ice age, as in fact no increase of the obliquity
can. In my former work I quoted an ingenious explana-
tion by Mr. Belt of how General Drayson's misconception
possibly arose (*Glacial Nightmare*, p. 363 n.).

In regard to the possible variation in the angle of obliquity
of the earth's axis to the ecliptic being very limited and
ineffective as a factor in creating a change of climate, no
question is, in fact, more firmly established. Sir Robert
Ball, whose championship of an astronomical theory of
an ice age is so unqualified, is quite at one with other
astronomers on this question, and is most emphatic in his
opinion that the variation in the obliquity of the axis is so
slight and must by itself have such a slight effect upon climate
that it is a negligible quantity. His words are: " Amid so
much that is changeable in the planetary system, it is for-
tunate that the obliquity of the ecliptic may for our present
purpose be regarded as practically constant ".

It is plain, therefore, that no possible variation or modifica-
tion either in the eccentricity of the earth's orbit or in the
obliquity of its axis, when taken by itself, can materially
affect the climate of the earth in the direction of creating an

ice age, and whatever effect the increase of either of these factors may have otherwise, it must tend to undo rather than create such an ice age.

Is it possible that such a change could be induced by combining these two elements in any conceivable way? First let us combine a high eccentricity and an enhanced obliquity. As we have seen, if we increase the eccentricity we also increase the total amount of heat received by each hemisphere annually, while if we increase the obliquity we secure that a greater relative amount of heat is received by the high latitudes during the year than would be received now. If we combine the two, therefore, instead of creating, or tending to create, an ice age, we must create a higher mean temperature in the high latitudes where the problem has really to be solved. By such a change we no doubt secure a greater contrast in each hemisphere in summer and winter, but the alternating conditions thus produced do nothing to produce an ice age. The effects of the colder winter must be regularly dissipated by those of the relatively hotter summer, and there can be no accumulation of cold. From this conclusion there seems no escape.

The only combination of the two elements in question which would create a colder mean climate is a small eccentricity and a small obliquity. Inasmuch, however, as the eccentricity is at this moment almost at its possible minimum, and it is clearly inoperative to create an ice age now, we may neglect that element, and we are reduced in calculating the effects of this combination to the simple problem of measuring the result of the minimum amount of obliquity upon the present mean annual climate of high latitudes. This we have already examined and found utterly wanting. The greatest possible variation in the obliquity is in fact so slight that Sir Robert Ball, who would be a most eager champion of such a cause if it were available, has to confess that it is really too small to be taken into account as an effective factor in inducing climatic change, and he consequently treats it as a virtually constant and invariable factor. We may take it, therefore, that no combination of these two elements which can be suggested will avail to alter the earth's mean temperature materially or to create an ice age.

2

Let us now shortly consider a third variable element in the world's motion. If the earth's axis were always exactly parallel to itself, each hemisphere would always have its summer and its winter when at the same parts of the orbit —that is to say, the hemisphere whose summer occurs now when the earth is nearest to the sun, *viz.*, the southern hemisphere, would continue to have it in perihelion for all time, while its winter would be in aphelion. With the other hemisphere, the northern hemisphere, this condition would be reversed, but in each case the conditions would remain constant : one would always have a short summer when nearest to, and a long winter when furthest from the sun, and the other would have the reverse.

Instead of always remaining constantly parallel to itself, however, the earth's axis has a slight and very slow wobbling motion (like the axis of a top when spinning), so that it generates a double cone with the apex of each at the earth's centre, and each pole in consequence, in addition to its other motions, generates a small circle with a sinuous outline round the pole of the heaven. This motion is due to the fact of the earth being a spheroid and not a perfect sphere, and having a great bulge round its equator, which causes the attractions of the sun and moon to vary in intensity upon different parts of its surface. These, again, vary as they pull together or against each other, and as the pull is over the equatorial region or over some other latitude. The results of this differential attraction are what are known as precession and nutation or nodding. I have described them in my former work (*Glacial Nightmare*, pp. 370-373). Their effect is not to alter the angle at which the earth's axis is inclined to the ecliptic. This is not materially affected, but the earth's axis is made to point continually in a different direction, so that when it has generated half the circle above named it will lean athwart its former position, and will thus gradually reverse the position of the two hemispheres towards the sun, so that the hemisphere which once faced the sun will necessarily face away from him.

The combined motion in question is a very slow one, and it results in the fact that every year the March equinox is about twenty minutes and twenty seconds earlier than it was the year

before, and in the course of time this and the corresponding autumn equinox will have made the whole circuit of the earth's orbit. The time required for this circuit is about 21,000 years.

In half of this, *i.e.*, about 10,500 years, therefore, the two equinoxes will change places, and the two seasons will be completely reversed. The hemisphere which had its summer when the earth was nearest the sun will now have its winter at the same time, and the reverse will be the case with the corresponding hemisphere; and this alternation will go on every 10,500 years. Precession and nutation when taken by themselves merely have the effect of changing the relative positions of each hemisphere in regard to climate every 10,500 years, so that the short summer and long winter in one will alternate with the long summer and short winter in the other. They have no other effect either in altering or distributing the heat received annually by the earth.

Inasmuch, however, as the variation in the eccentricity of the earth's orbit does not happen during the same cycles as these alternations, but during cycles of much greater extent, so that several of the latter are contained in one of the former, it also follows that sometimes the shortest and sometimes the longest summer possible will coincide with the earth's greatest proximity to or its greatest distance from the sun respectively, with the reverse positions in regard to the complementary season, and this will of course sometimes happen in one hemisphere and sometimes in the other. This affords in the one case the least possible contrast, and in the other the greatest possible contrast between the seasons; and Sir R. Ball has argued that this contrast when thus enhanced to the highest degree has a distinct tendency to create an alternation of glacial and mild climates. This is in effect Sir R. Ball's case.

He urges that the fact is plain enough if only we realise a critical factor which had been previously overlooked. He affirms that there had hitherto been a crass and fundamental error among astronomers in regard to the direct effect of the obliquity of the earth's axis upon the distribution of heat over each hemisphere during summer and winter respectively, and in regard to which he especially charged Herschel and

Croll with what he calls absolute blunders in the simple
mathematical questions which are involved. These charges
are invalid, and ought never to have been made against
such men from Adams' chair at Cambridge, for they do not
refer to some intricate and involved issue where a mistake
might easily arise, but to one of the simplest and most
elementary matters in astronomy and meteorology, namely,
the question of whether the amount of heat received by
each hemisphere is the same in summer and winter, or is
greater in the former than the latter. Dr. Ball charges
Herschel, Croll and other writers with advocating the former
view, and with, as he says, thus "falling into an error by which
they have been sadly led astray," and speaks of it as the
fundamental error which has vitiated the astronomical theory
of the Ice Age as sometimes presented, and which it was the
chief object of his book to correct (*The Cause of an Ice Age*,
2nd edit., p. 89). As a matter of fact, in paragraphs 336
and 337 of the fifth edition of his *Outlines of Astronomy*,
Herschel has stated the law of the distribution of the sun's
heat between summer and winter in plain and accurate
terms, showing that he perfectly realised the inequality of
the heat received by each place during the two seasons. In
a subsequent unfortunate phrase, he says that *one half of the
sun-heat is received in summer and the other half in winter.*

Seizing upon this slip (which is very like a slip of Dr.
Ball's own, where he says *our hemisphere was once covered with
ice (op. cit.*, 1st edit., p. 2)), and entirely ignoring what Herschel
had already written in the earlier part of his book, Sir R. Ball
charges the latter, whose memory is very dear to some of us,
with ignorance of a most elementary kind. He then charges
Croll with having copied and followed Herschel in his mis-
take, and with having thus entirely misapprehended the
problem he tried so hard to solve. This in regard to
Croll was a most wanton attack. Nowhere in his works
does Croll make the statement that one half of the heat
which reaches either hemisphere reaches it in summer and
the other half in winter, and it is perfectly plain that he
knew the substantial truth about it. On page 55 of his
Climate and Time he tells us how "the total duration of sun-
shine in winter and in summer in the latitude of Edinburgh

is about 4 to 7. The quantity of heat received during winter is scarcely one-third of that received in summer" (*op. cit.*, pp. 86, 87). This shows that Croll was perfectly aware of the disparity which Dr. Ball claims as his prime discovery, and as the key of the glacial question. These facts were pointed out by Mr. Noble in a letter to *Knowledge*, in which he vindicated Croll's reputation. In the second edition of his *Cause of an Ice Age*, Dr. Ball, while maintaining his unfair criticism of Herschel, somewhat qualified his charge against Croll, but in an ambiguous, disingenuous and unchivalrous manner (*The Cause of an Ice Age*, 2nd edit., pp. 118, 119).

Dr. Ball having charged Herschel and Croll with elementary blundering, proceeded with great parade to publish what he urged was the real key to the whole matter, which every one before himself is supposed to have neglected and overlooked, namely, a calculation of the exact proportions in which the heat received by each hemisphere is distributed between summer and winter. This he showed is nearly in the proportion of 63 to 37 (or, as Professor Darwin puts it more accurately, in the proportion of 5 to 3), and he proceeds to exalt these numbers into a most potent fetish. He tells us they are the key to the whole question and that we must never lose sight of them, and claims his discovery of them as the one novelty in his book and its application as his excuse for publishing it. It was immediately pointed out to him however by Professor Darwin that the law in question had already been published by Wiener in a memoir entitled " Ueber die Stärke der Bestrahlung," in the *Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie*, vol. xiv., 1879, p. 129. The actual numerical ratio as stated by Wiener was 3·93768 and 2·34550.

In 1892 Sir R. Ball brought out a second edition of his work. In this, and also in a note to *Nature*, he acknowledged the justice of Wiener's prior claim, and withdrew his pretensions to having discovered the law in question. He now had to find a fresh reason for publishing his book. On page 114 of this new edition, after admitting Wiener's claim, he goes on: "I have, however, never met with any application of this theory," and confesses his surprise that he had not observed in any of the writers with whom he was acquainted any consciousness

of the existence of such a law, which he says is "so pregnant with significance that the theory of the Ice Age cannot rightly be understood unless by those who are acquainted with the facts to which it gives expression".

If this means that no one before Wiener had made the precise calculation upon which the exact numbers 63 and 37 are based, and that no one had ever applied these precise figures, this may be true, but it is quite certain that astronomers had long been aware of the elementary truth of the disparity of the summer and winter temperature in each hemisphere, and Croll, whose *Climate and Time* had been published four years when Wiener's memoir appeared, and Haughton in his paper in the *Transactions of the Irish Academy* for 1881, had arrived at roughly the same proportion. To them, as to others, however, it does not seem to have had any real significance at all, nor to have been of real assistance in any way in solving the problem of an ice age.

The more precise numerical results arrived at by Wiener and Ball are not disputed. They are unquestionably perfectly sound in themselves. What immediately struck people, however, on their publication by Dr. Ball, was their utter and complete irrelevancy to the whole problem and its discussion. It was at once seen, and Dr. Ball did not dispute it (*op. cit.*, 2nd edit., p. 121), that the numbers which he relied upon as marking the proportions of sun-heat received in summer and winter respectively are virtually *constant numbers*, and true at all times, and, being so, can not be themselves the foundation and cause of *secular variations in climate*. They are as true now as they were in any other period of the earth's history, or rather, under any other *possible* conditions in the obliquity of the earth's axis, and being a constant and unchangeable factor cannot cause an ice age at one time and a warm one at another.

Nor do they cease to be irrelevant when combined with the varying length of the seasons induced by a variation in the eccentricity of the earth's orbit. Neither separately nor in combination do these magical numbers supply us with any additional heat from the sun or diminish the amount he supplies us with.

Nor do they affect the relative length of the seasons or their differential temperature concurrently with a changing

eccentricity any more than adding the same sum to each side of an equation affects the result.

Dr. Ball's argument is as follows :—

In the time of greatest eccentricity 63 per cent. of the heat which reached one hemisphere was poured upon it during 166 days and 37 per cent. was poured upon it in 199 days, and in the period of least eccentricity the same proportions of heat were poured upon it in 179 and 186 days respectively; and he adds: "A little consideration will show that these figures must import wide climatic differences between the different seasons " (*op. cit.*, p. 102); and he speaks of " this inequitable distribution as bespeaking a climate of appalling severity—an ice age in fact ".

This assuredly involves a complete fallacy. Let me answer him in a very simple way. Instead of quoting the magical figures with which he professes to conjure, let me state the proposition thus: "In the time of greatest eccentricity *precisely the same percentages of heat which now reach either hemisphere in summer and winter* will be poured upon it in 199 days and 166 days respectively, instead of in 186 and 179 days as now ". It will be now seen that Sir R. Ball's famous discovery is absolutely irrelevant to the issue, and that the problem simply remains in precisely the position it was in before.

The fact is that Dr. Ball himself treats his own figures as irrelevant, for, notwithstanding the parade with which he announces them, when he comes to define the problem on a later page he entirely leaves them out. He there in fact postulates two conditions only as the essence of the astronomical theory: " First a high eccentricity, and secondly that the line of equinoxes shall be perpendicular to the major axis of the earth's orbit, so as to make the inequality between the duration of the two seasons as great as is compatible with the eccentricity in question "; and he adds: " The critical magnitude which decides whether a certain year is a glacial year or not simply depends, according to the astronomical theory, on the differences between the lengths of the seasons in that year " (*op. cit.*, 2nd edit., pp. 105, 106 and 151, 152). This is precisely the astronomical theory which other astronomers have repudiated as fallacious and invalid, and which they have all, from

Arago to Croll, shown to be so, because of the compensation which must occur whatever the mode of distribution when the sum of annual heat is the same.

I altogether fail to understand the process of reasoning by which Dr. Ball has reached his conclusion. No amount of disparity in the length of the seasons can create an ice age so long as precisely the same heat, or rather, so long as a slightly increased amount of heat, reaches the earth in a year. No juggling with figures, no possible distribution of this heat known to us will do it. If the winter consists of a larger number of colder days, the summer will consist of a correspondingly shorter number of warmer days, and the fiercer heat of the shorter summer must undo the work of the longer winter. Granting a uniform rate of terrestrial radiation, the thermal and dynamical effects of the same annual sum of heat received must be identical. The compensation, as Arago says, is mathematically complete.

This is amply conceded by Croll. The only effect of the causes discussed is to give one hemisphere an alternation of more contrasted summer and winter than the other. But the sum total of accumulated effects at the end of the year going to produce a glacial or torrid *period* respectively must be *nil*, since precisely the same heat is received and used and stored under either condition. I am at present, of course, excluding all kinds of meteorological considerations, and all questions of a varying rate of radiation, and treating the question purely as an astronomical one. They are excluded by Sir Robert Ball himself, who claims to have founded and fixed an astronomical cause quite competent to account for the phenomena without the help of Croll's meteorological machinery. It seems to me that until and unless we can materially either increase or diminish the number of thermal units received by either hemisphere from the sun in a year, we cannot directly attribute to an astronomical cause alone a variation in terrestrial climate extending over a geological period.

It is extraordinary that this should not have occurred to Dr. Ball, and that in view of the stupendous difficulty of his postulate, he should not have also thought it necessary to elaborate his argument on this point into some kind of proof, instead of leaving it as an *obiter dictum* which, when analysed,

is utterly wanting. It is also very extraordinary that when in search of some analogy with which to illustrate his position he should have pitched upon one so absolutely ridiculous and inept as the one he has chosen on page 110 of his work, in which he compares a summer sun melting an ice sheet at different rates under different conditions with a horse alternately gorged and depleted with food, an illustration which savours of the logic of circle-squarers and men of that ilk rather than of so very able a person as Dr. Ball.

It is perfectly plain, therefore, that by the use of his vaunted magical figures Sir R. Ball has completely failed to establish the astronomical theory of an ice age and to meet the fatal objections to which it is liable. He has added no new effective factor to those which previous inquirers had analysed and rejected. Nor has he anywhere in his book carried out the brave words of his preface, and shown us how and in what way his mystical numbers 63 and 37, or in fact any contrasted but fixed proportions of heat and cold such as he quotes, have any real potency at all in creating an ice age. He has nowhere gone beyond an *obiter dictum*, and *obiter dicta* which cannot be analysed into sense are not recognised as arguments in science as they are in theology.

This is not all however. Sir R. Ball has in fact utterly mistaken the conditions under which the sun distributes its heat over the earth, and the real effect of that distribution in modifying climate. The numbers 63 and 37 with which he tries to conjure, no doubt represent fairly accurately the relative proportions of heat received by each hemisphere in summer and winter respectively, but we must always keep in mind that this is true only of each hemisphere when considered and treated *as a whole*.

If we divide each hemisphere into zones we shall find that these figures entirely cease to be true, except for a hemisphere as a whole, and for the two parallels of latitude marked by 36° of north and south latitude respectively. For every other parallel of latitude they are quite misleading and untrue, and they have in fact misled Sir R. Ball himself. It is an elementary truth that " at or near the equator the heat received in each season is the same, while at the poles all the heat is received in summer; the distribution of heat

between the seasons, in fact, varies with the latitude " (Becker, *Amer. Journ. of Sc.,* xlviii., p. 97). The true method of facing the problem is not that adopted by Sir R. Ball at all. The true method had been pointed out long before in Mr. Meech's paper already cited (*op. cit.,* p. 51). He showed that in order to arrive at the effect of a varying obliquity of the axis on climate it is not enough to lump all the heat received by each hemisphere together, but if we are to test the problem properly, we should examine separately the various zones into which that heat is distributed, and has been distributed in former times, and he tests it by a critical example.

According to Meech's table, between 8200 B.C. and 1850 A.D., the change in the sun's annual intensity has been :—

Latitude.	Difference in thermal days.	Latitude.	Difference in thermal days.	Latitude.	Difference in thermal days.
0	− 1·65	80	− ·96	60	+ 2·11
10	− 1·58	40	− ·22	70	+ 5·52
20	− 1·82	50	+ ·68	80	+ 7·18
				90	+ 7·64

That is to say, in 8200 B.C. at the equator the annual intensity of heat was 1·65 less than it is now, *i.e.,* the equatorial zone was so much cooler. From lat. 35° to 50°, comprising the temperate zone, it was virtually the same as it is now, but above 50° of latitude the annual intensity of heat was greater in an augmenting degree towards the pole, where it culminated in a climate comprising seven to eight thermal days in excess of that now existing. In other words, the north and south poles 10,000 years ago received twenty rays of solar heat respectively in a year where they now receive but nineteen, as the direct result of a varying obliquity of the earth's axis, so that in the particular latitudes where glacial conditions are chiefly postulated the earth's astronomical climate instead of having been colder was *pro tanto* hotter.

While Meech treated the problem as it affects the *annual* range of climate caused by a varying obliquity, Dr. G. Pilar in an interesting memoir entitled *Ein Beitrag zur Frage ueber die Ursache der Eiszeiten,* published at Agram in 1876, page 52, calculated and printed a table showing the difference between *the summer and winter temperature* of each fifth degree of latitude between the equator and 65 N. and S. lat. in both hemispheres respectively, as it is now and as it was in the time of greatest eccentricity with winter in perihelion

and summer in aphelion in the northern hemisphere and the reverse conditions in the southern hemisphere. This table, which is most useful, shows that a quarter of a century ago the true conditions of the problem had been faced by the Croatian investigator, and it seems a strange thing that his paper should have been overlooked and its significance not been appreciated. Twenty years later Becker in the paper above quoted, which was published in August, 1894, and Culverwell who read his paper about the same time, made similar calculations in regard to the divergence between the summer and winter climate under varying conditions in different zones, a problem which, as the former says, is independent of the length of the seasons, being proportional to the change of the earth's longitude in its orbit.

Their results all show us that it is a misleading method to lump all the zones in each hemisphere together instead of treating them separately when we are comparing ancient climates with modern ones. To show how misleading Sir R. Ball's lumping method is, we only need point out how, after postulating his magical figures, he in fact proceeds to argue as if the proportions he had worked out for a whole hemisphere (which are true enough when so defined) are true of the latitude of Britain. Now, if we examine the tables which follow, and which I owe to Mr. Culverwell, we shall find that in the latitude of Britain, that is, roughly, in latitude 55°, which is the mean between 50° and 60° north latitude, the proportions of sun-heat at present received in summer and winter respectively are as 909 to 269 (Croll roughly states them as three to one in the latitude of Edinburgh), and not as 63 to 37 at all.

In order to test the validity of Dr. Ball's methods, Mr. Culverwell proceeds (on the basis of these corrected numbers, 909 to 269, and allowing Sir R. Ball his full amount of variation due to eccentricity, *i.e.*, thirty-three days) to calculate the result with a very extraordinary effect. As he says, at the moment when Dr. Ball's book was written the amount of heat received in Dublin in the 199 coldest days of the year was barely two-thirds of the amount (37 measures) which is supposed to have been received during the same number of days in the winter of the Ice Age, and the people of

Dublin ought, therefore, now to be suffering under a still more intolerable climate, one of far more *appalling severity* than the actual glacial period (see *Geological Magazine,* February, 1895, p. 60). Can anything better show the absurdity of the whole argument than a test like this ?

It is, however, not the only critical test of the kind which can be applied as a touchstone to Sir R. Ball's position. In another place in his book Dr. Ball proceeds to argue that the effect of combining his magical figures with the results of varying eccentricity is, that in the period when that eccentricity was greatest there would be an increase of the mean annual range of temperature in Great Britain of from 20° F. to 28° F., which would create glacial conditions here. " Surely," says Mr. Culverwell, " this is a *reductio ad absurdum* of the whole theory. There is hardly a climate in the northern hemisphere where the range is not much greater than 28° F. In latitudes 40°, 50° and 60° in America the ranges are 50° F., 65° F. and 75° F. respectively, but we have no ice age there. Why should a range of 28° F. in Great Britain be supposed to produce an ice age? The reason given is (p. 131) : 'It is to be observed that, generally speaking, the coldest places are those of the greatest mean annual range. We are therefore *entitled to infer* that the effect of such a change in the eccentricity as we have supposed, would be to increase the range, lower the temperature of the hemisphere, and thus induce the glacial period.' "

" How are we to discuss such an assertion ? " asks Mr. Culverwell. " It amounts to this, that *without examining what the alleged connection between annual temperature and annual range is, and without any previous attempt to ascertain what temperature is necessary to bring about an ice age,* we are ' entitled to infer ' that a range of 28° F. in Great Britain would induce the glacial period ! So far from that being the case, the most cursory examination of the far greater range of temperature in non-glaciated regions at present, seems to entitle us to infer that the supposed increase of range would be quite powerless to effect the desired, or indeed any very marked, change in climate " (*Geological Magazine,* February, 1895, p. 61).

Mr. Culverwell was not content with this complete *reductio*

ad absurdum of Dr. Ball's method and results. He went on to give us a systematic analysis of the whole problem on truly inductive lines, and to prove remorselessly not only that the particular astronomical theory of Dr. Ball is untenable, but that all purely astronomical theories are and must be so.

This he has done by first analysing the actual amount of sun-heat at present being received in summer and winter respectively, not over a whole hemisphere nor over the whole of the zone between the tropics and the poles lumped together, but over different latitudes, as Pilar had done twenty years before for a less extended area; and secondly, by calculating the corresponding amounts of sun-heat which would be received on the same latitudes in summer and winter during the time of greatest possible eccentricity with the winter in aphelion, and then comparing the two and so discovering the actual amount of change produced.

The following table represents the results of Mr. Culverwell's calculation of the present number of heat units which fall on each latitude at intervals of five degrees:—

Latitude.	Winter.	Summer.	Latitude.	Winter.	Summer.
0	961	943	50	345	947
5	915	971	55	269	909
10	866	997	60	197	868
15	808	1016	65	135	840
20	752	1026	70	79	821
25	694	1030	75	40	806
30	634	1026	80	18	793
35	569	1016	85	5	780
40	498	997	90	0	766
45	428	973			

Taking it in another way, he finds that the total sun-heat received in summer is to that received in winter on each five degrees of latitude in the following proportions:—

Zone.	Summer.	Winter.
0°— 5°	478	469
5°—15°	987	857
15°—25°	964	707
25°—35°	886	550
35°—45°	764	381
45°—55°	612	269
55°—65°	434	100
65°—75°	286	27
75°—85°	84	9

The merest tyro who uses the conventional language of everyday life speaks of the perpetual summer of the tropics, and this table shows that he is virtually right. Of the total amount of heat which falls upon the earth one half (the actual proportion as deduced from the figures is $\frac{12589}{25033}$) falls upon it in the zone between the equator and the parallels of 30 north and south which mark the tropics, and within this zone the proportion of summer to winter sun-heat received is not 63 to 37, as Dr. Ball argued, but about 70 to 56 or 63 to 50, and if we take latitude 40° as more nearly in a rough way bounding the area on each side of the equator where so-called glacial phenomena do not occur, we shall find that the total sun-heat received amounts to $\frac{15712}{25053}$ or three-fifths of the whole, in the proportions between summer and winter of roughly 34 to 26. In both cases the proportion is very different to that of the mystical numbers referred to by Sir R. Ball.

According to the rectified numbers, therefore, about three-fifths of the total sun-heat received on the earth falls between the equator and the parallels of 40 north and south latitude, leaving only two-fifths of the total sun-heat to supply the other 60° of latitude in each hemisphere; and this must always have been the case. Now these two-fifths, and not the total sun-heat received, are the real element in the problem to be solved, since it is between latitudes 40° and the poles that the phenomena of the so-called glacial age alone present themselves.

If we want to ascertain how this two-fifths is distributed in summer and winter respectively in the zones between the poles and latitudes 40°, it is clear we must subtract three-fifths or 60 per cent. from the whole heat received on each hemisphere in the proportions of 34 to 26, from Sir R. Ball's mystical figures. We shall then find the result to be that the amount of sun-heat so received is as (63 — 34) is to (37 — 26), that is to say, as 29 to 11. These then, and not 63 to 37, are approximately the numbers which mark the present proportion of sun-heat in summer and winter respectively in the zones of the earth where glacial phenomena are alone supposed to have occurred.

Having thus corrected Sir R. Ball's figures, let us proceed to the more concrete problem of actually comparing the

present condition of things as thus attested in those zones, where glacial phenomena are supposed to be chiefly forthcoming, with the condition of things during the period of greatest eccentricity.

Mr. Becker and Mr. Culverwell have both attacked the problem, and their results, which differ only slightly in accordance with slightly differing postulated values for eccentricity, obliquity, etc., are very striking. Becker has tabulated his results thus:—

HEAT RATES FOR NORTHERN SUMMER.

Lat.	P.	N.	X.	B.
0	0·9397	0·9592	1·0626	0·9552
10	0·9945	1·0150	1·1245	1·0144
20	1·0216	1·0427	1·1552	1·0457
30	1·0208	1·0419	1·1543	1·0489
40	0·9930	1·0135	1·1229	1·0245
50	0·9411	0·9605	1·0642	0·9762
60	0·8721	0·8901	0·9862	0·9111
A	0·8270	0·8441	0·9852	0·8768
70	0·8115	0·8283	0·9176	0·8587
80	0·7869	0·8082	0·8898	0·8381
90	0·7798	0·7959	0·8818 .	0·8326

HEAT RATES FOR NORTHERN WINTER.

Lat.	p.	n.	x.	b.
0	0·9797	0·9592	0·8785	0·9552
10	0·8955	0·8768	0·8080.	0·8698
20	0·7869	0·7705	0·7057	0·7610
30	0·6577	0·6439	0·5898	0·6326
40	0·5127	0·5019	0·4597	0·4893
50	0·3583	0·3508	0·3213	0·3384
60	0·2051	0·2009	0·1840	0·1901
A	0·1164	0·1140	0·1044	0·1198
70	0·0821	0·0804	0·0736	0·0764
80	0·0198	0·0194	0·0177	0·0182

In these tables P and p stand for present obliquity, eccentricity and length of seasons.

N and n stand for present obliquity, 23·27, and zero eccentricity.

X and x stand for present obliquity, eccentricity = 0·0745, and greatest difference of seasons.

B and b stand for obliquity = 24·36, and zero eccentricity.

A represents the latitude of the arctic circle, or 66·33 in all cases except those of B b, for which A = 65·24.

Let us now turn to Mr. Culverwell. He has tested the problem in three different ways.

First.—Following Croll's idea, he has calculated what latitudes now receive the same winter sun-heat as was received during the period of most extreme eccentricity with winter in aphelion in the areas marked by the parallels of 40, 50, 60, 70, 80 and 90, and he finds that on midwinter day the parallels of 43, 52, 61, 70, 80 and 90 do so. Hence, he says on Croll's hypothesis, we should expect the midwinter temperature of 40°, 50°, etc., in the glacial epoch to have been about the same as those of 43°, 52°, etc., at present.

Secondly.—Following Ball's method he has calculated what latitudes now receive the same *daily average* of sun-heat in winter (*i.e.*, from equinox to equinox) in 179 days as was received during the 199 corresponding days when the eccentricity was most marked in latitudes 40, 50, 60, 70, 80 and 90, and he finds that it would be latitudes 43·3, 52·4, 61·7, 71·3, 81 and 90.

Thirdly.—He has calculated what latitudes now receive in the 199 *coolest days* the same total amount of sun-heat as were received during the winter of 199 days in the period of greatest eccentricity by latitudes 40, 50, 60, 70, 80 and 90, and finds they would be latitudes 44·2, 54, 63·5, 74 and 84·5.

The general conclusion drawn by Mr. Culverwell from these figures is that the temperature of latitude 50° in the supposed glacial epoch cannot, so far as direct sun-heat is concerned, have been as much as from 3° F. to 5° F. lower than its present temperature, and similar reasoning applies to the other latitudes between 50° and 70°. Thus the conclusion is that the midwinter sun-heat temperature from 50° N. to 70° N. due to diminished winter heat in the epoch of greatest eccentricity cannot have been as much as from 3° F. to 5° F.

Translating this into a more concrete shape, it means, as Mr. Culverwell says, that the midwinter sun-heat temperature of that part of England which is included between 50° and 55° cannot have been as low as that of the region between York and the Orkneys is now. "The foundation of the astronomical theory thus breaks down completely. It requires us to suppose that the same quantity of sun-heat as that which is now falling on Yorkshire and gives us a mild and equable climate will, if it falls on Cornwall, produce an ice age; or

again, that if the present winter sun-heat at Oxford were to be reduced to the amount which now falls at Melrose Abbey, such deluges of snow would cover Oxford that a *summer heat far greater than that now received there* would be unable to melt it. . . . . Observe, *a summer heat far greater than that now received at Oxford ;* for in the 166 days of the short hot summer of great eccentricity, Oxford would receive as much summer heat as is now received in an equal time by latitude 35°, say by Tangier or Algiers " (Culverwell, *Geol. Mag.*, January, 1895, pp. 7-9). It should be added that the numbers here given all suppose the greatest possible eccentricity, a contingency which happened an appalling time, namely, 840,000 years ago. If they are applied to the last great eccentricity, which, according to Leverrier, happened 100,000 years ago, they must be reduced by a fourth.

Sir R. Ball was present at the British Association meeting at Oxford when Mr. Culverwell read his paper embodying these arguments, and spoke upon it, but only to make some ill-timed and irrelevant jokes. The substance of the paper was subsequently printed in *Nature*, and in the *Philosophical* and *Geological Magazines.* No notice of its criticisms was taken for a considerable time, not in fact until I ventured to write some strong and perhaps too personal letters in *Nature* detailing some of the circumstances under which Dr. Ball's book was originally published and continued to be issued.

These letters had the effect of drawing first from Prof. G. Darwin and secondly from Sir R. Ball himself some remarkable communications. The former, who had given some countenance and support to *The Cause of an Ice Age*, wrote to say that he could not see any mistakes in Culverwell's argument, and went on to say *that he was reluctantly compelled to give up the astronomical explanation of an ice age.* Sir R. Ball's letter is equally welcome. In it he also apparently surrenders his former views, and his faith in the cabalistic numbers 63 and 37 and all he built on them, and completely accepts Culverwell's results *in so far as an astronomical cause pure and simple is concerned.* He goes on, however, to say, that granting them to be true, we must remember that we cannot shut off zones of climate by solid partitions, and that the climate of every zone is the result not merely of the sun-

heat directly received on that zone, but of the heat brought in by convexion, by winds or currents of water from elsewhere.

This is assuredly a complete surrender of Sir R. Ball's original pretension that he had discovered an astronomical theory which would explain the Ice Age without having recourse to extraneous help or extraneous forces. He now in fact fell back upon winds and water currents, upon convexion and transfer of heat, etc., which formed the main machinery in Croll's workshop. Croll argued, as we saw in our former volumes, with great ingenuity and force, that it was only by summoning these meteorological agencies that we can make the astronomical theory work. It was Dr. Ball's purpose and aim to dispense with them, and the main purpose of his book to show that by astronomical causes alone he could explain and illustrate the Ice Age, and to show that Croll and Wallace were mistaken in deeming these supplementary causes necessary. The surrender of Sir R. Ball on this cardinal point, and the concurrence of Prof. Darwin in the views of Culverwell, take away, I believe, the very last physicist and astronomer who have been found to maintain an astronomical theory of an ice age pure and simple.

Sir Robert Ball's rhetoric probably condenses the very best case that can be made out for it—and what an utter breakdown this means. It seems to me that no one can read the cases *pro* and *con* without subscribing to Mr. Culverwell's dictum that the theory is " but a vague speculation, clothed indeed with a delusive semblance of severe numerical accuracy, but having no foundation in physical fact, and built up of parts which do not dovetail one into the other " (*op. cit.*, p. 3).

I cannot close this chapter without expressing the profound regret I feel that Sir Robert Ball should have fathered a theory which had been repudiated by every other astronomer, and have thus given a prestige to it which has greatly misled some ill-informed men anxious to find a mathematical and irreproachable basis for their extravagant conclusions.

The field Sir R. Ball chose for his rash experiment upon the credulity of geologists was one to which he had been previously a stranger, and where he was dealing therefore with unaccustomed facts and modes of things. It is a remarkable Nemesis which has awaited him, mainly at the

hands of one of his own brilliant pupils, Mr. Culverwell. Although he caused some eager writers to take a retrograde step, and has misled a good many students (perhaps as much by his subsequent reticence after his arguments were exploded as by his original pronouncement), and his book has had a passing reputation and was once quoted with some confidence, this is no longer so. There are not any geologists of any reputation known to me who subscribe to his conclusions now. So far as I know, the astronomical theory of an ice age pure and simple is dead ; as dead as Croll, its keen opponent, declared it to be a quarter of a century ago. Unfortunately heresies have a habit of long outliving their reputation. They get enshrined in otherwise reputable memoirs and manuals used by students ; and thus a succession of minds are imbued with the leprous taint, which is liable to unexpected resuscitation. The place the theory fills, and the hesitating patronage extended to it in such works as Dr. James Geikie's last edition of his *Ice Age*, are measures of the harm it has done and is still doing.

Note.—I ought to add another argument to those contained in the foregoing chapter, and which escaped me until it was in type. It is a curious fact that the differential astronomical elements affecting the present summer and winter climate of the two hemispheres are very nearly a mean between those postulated by Sir R. Ball during his glacial and interglacial periods respectively. If the causes he quotes sufficed to create intolerable glacial periods, they must also have caused not temperate but almost torrid climates during his interglacial periods, and this seems to me to be an inevitable conclusion from all astronomical theories of an ice age pure and simple. Are those geologists who listened to the charmer when he told them that " the astronomical theory removes the glacial period from the position of a ' catastrophic ' phenomenon, and places the ice sheet as an implement at the disposal of the geological uniformitarian, to be used with the other agents with whose powers he is already familiar," prepared to accept these *torrid* interglacial periods as cheerfully as they accept the glacial nightmare ?

CHAPTER II.

A GLACIAL AGE AS AN INDIRECT RESULT OF INCREASED ECCENTRICITY OF THE EARTH'S ORBIT—THE METEORO- LOGICAL THEORY OF CROLL AND HIS FOLLOWERS.

"When science is used as a means of education, it is of high importance that the reasoning placed before the reader should be sound" (Rev. E. Hill, *Geol. Mag.*, 1880, No. 10).

In the previous chapter I have subjected the astronomical theory of an ice age pure and simple to analysis, and have collected the various arguments which go to show that not only has its latest champion, Sir R. Ball, utterly failed to substantiate it, but that it is clearly and palpably quite incompetent to explain any but the most trifling changes of climate, a view on which apparently every astronomer and physicist is now agreed. It is plain that if we are to seek for a cause competent to explain the Ice Age, it must be some cause other than " the relative position of the sun and earth under their present laws of motion ".

If we are to find such a cause which shall be consistent with the demands of inductive science, we must therefore either go outside the astronomical conditions here referred to and supplement them by some other factor, or we must postulate some astronomical changes in the course of geological time which have interfered with the ordinary and normal course of nature. Let us first turn to the former alternative.

In Sir R. Ball's analysis of the glacial problem he left out of account any effects due to a variation in the radiation of heat from the earth's surface, etc. In stating his problem he says : " It is no doubt true that there must have been many circumstances which tended to modify the climatic changes that have followed from astronomical causes ; it is no doubt true that many difficult subsidiary questions arise. *There are*

questions about the laws of radiation and of cooling, questions as to the distribution of land and water, and questions as to the effects of winds and clouds and ocean currents. *But none of these details, however important they may be, should be permitted to obscure the broad features of the question."* He accordingly ignored what he called "subsidiary questions". In the previous chapter, where I have tried to meet him on his own ground, I have done the same, but it is clear that if I am to make my reply complete and conclusive, I must face them now, and I will begin with the question of the general laws of radiation of heat. I shall, as before, not shrink from using some very elementary language, in order to make a not easy problem as plain as I can.

When heat passes from one portion of matter to another through an intervening medium which is transparent or diathermatous to it, it is said to pass by radiation, and is known as radiant heat.

As Tait says, "we have absolutely no proof that radiation from the sun is in any of the forms of energy which we call heat while it is passing through interplanetary space. That it is a form of energy, and that it depends upon some species of vibration of a medium we have absolute proof. The energy of vibrational radiation is a transformation of the heat of a hot body, and can again be frittered down into heat, but in the interval of its passage through space devoid of tangible matter it is not necessarily heat." Whatever it be, when this radiant energy meets with tangible matter, it is obviously and plainly disclosed to us as heat.

"According to theory," says Balfour Stewart, "all kinds of radiant heat, whether they have issued from a source of high or from one of low temperature, are in presence of an absolutely black surface at once and entirely converted into absorbed heat" (*Nature,* xxx., p. 191). Inasmuch, however, as no surface is absolutely black, a portion of the radiant energy which is intercepted by all tangible matter is reflected back again into space and the balance alone is absorbed, and this in proportion to the varying absorbing capacity of the matter in question. With reflected energy, we have nothing to do at present. Let us turn to that which is absorbed.

This behaves in different ways. That portion which is not

retained by the surface to warm itself, and is not again radiated back into space in the form of invisible heat, goes to warm the adjoining portions of matter. If the surface in question be a solid, and therefore molecularly a fixed one, this latter portion passes on to its contiguous continuation by what is called *conduction*; if, on the other hand, the surface is a liquid or a gaseous one, and its molecules instead of being fixed are mobile, the process in question is more complicated. Partially it loses its heat by conduction between one layer of liquid or gas and the adjoining one, as in the case of a solid, and partially by the internal movement of its molecules carrying the heat elsewhere, which process is known as convection. In addition to the heat absorbed by a liquid in raising its own temperature, and that conveyed elsewhere by conduction and convection, another portion is converted into what is known as latent heat, of which more presently. The rest is radiated into space.

Limiting our attention for a moment to the secondary radiation here mentioned, it was for a long time thought, on the authority of Newton, that the rate of cooling of a body subject to any constant cooling action was " proportional to the excess of the temperature of the body above that of the medium in which it was immersed " (Preston, *On Heat*, p. 445). This is fairly accurate for small differences of temperature, but for differences exceeding 40° or 50° C. it has been found to deviate very considerably from the truth. Eventually Stefan (*Sitz. d. K. Acad. d. Wiss. in Wien*, vol. lxxix., 1879) established from the results of Dulong's and Petit's experiment that the total radiation emitted by any body is proportional to the fourth power of its absolute temperature. Although this result was questioned by Bottomley, it seems to have been firmly established by the researches of Boltzmann and Gatilzene, which showed that Stefan's law follows theoretically from the law of thermo-dynamics (Preston, *op. cit.*, pp. 458, 460). It is obvious that the correction of Newton's law by Stefan imports a new factor into the problem we are discussing, and as we shall see presently it vitiates a great deal of the reasoning of Ball and Croll on the subject. Let us for the moment limit ourselves, however, to one particular issue only. If the rate of radiation were, as they supposed, the increase of the eccentricity of the earth's orbit, it would affect

the earth's temperature in two ways. In the first place, when at its maximum it would enhance the total annual heat received by the earth over and above its present supply by thirteen thermal hours. This increase of the sun's heat must be reduced somewhat in order to satisfy the operation of Stefan's law. This element, however, under any circumstances, is too microscopic in its amount to detain us, and is of merely academic interest.

The other element is of somewhat greater importance, namely, the convergence of the same amount of heat into a summer of 166 days at the time of maximum eccentricity instead of a summer of 179 days as now, and the corresponding dilution of the winter's heat by spreading it over 199 days instead of 186 days as now. It has been thought that the application of Stefan's law to such a case would considerably modify the result. As Mr. Culverwell reminds me, the enhanced momentary accession of heat in such a case would lead to an enhanced radiation at a much greater rate in accordance with Stefan's law. This is perfectly true, but it seems to me that the very same law which operates to thus minimise the summer heat, would operate in the opposite direction and in the same proportion to mitigate the winter's cold, and there would consequently be a complete or almost complete compensation. If the higher temperature of summer caused a higher *rate* of radiation, the lower temperature of winter would give a correspondingly lower *rate* of radiation in that season, and if there would be a somewhat lower summer temperature than otherwise in consequence, there would be a correspondingly somewhat higher winter temperature to need mitigating. Apart from this compensation, the numerical and quantitative results obtained by Pilar, Becker and Culverwell are so conclusive that the total possible effects of greater eccentricity of the orbit would affect the climate only very slightly, that we could, even if it did not exist, make a present to the champions of an ice age of all the advantages supposed to accrue from the differential character of the law of radiation as now ascertained without really affecting the issue.

Stefan's law makes more complete the conclusive case urged by Croll against Adhemar's glacial theory as based on differential radiation (see *Glacial Nightmare*, pp. 380-384).

Let us now turn to the *meteorological* causes so cavalierly ignored in his original pronouncement by Ball, and to which he subsequently turned, namely, those urged by Croll. Croll, while strongly and emphatically denying the possibility of any astronomical cause pure and simple as the cause of an ice age, argued that a combination of extreme eccentricity in the earth's orbit with a winter in aphelion would set in motion certain great meteorological agencies which would in turn produce the ice age in question. This view was for some time very widely held among geologists. It still survives, I believe, among some Scotch geologists, and Sir R. Ball himself, in qualifying his original position, has apparently fallen back upon it.

Croll was no doubt the most ingenious, acute and able champion of an ice age who has yet appeared. He was a good mathematician and a learned physicist, but he was also the champion of most fantastic causes, and unfortunately he was also a member of the Geological Survey. Unfortunately, I say, because his views, like those of Athanasius, whether wise and sound or only ingenious and clever, stood for something more than themselves, and came to be the pronouncement of a dominant and official geological school, namely that of the geological surveyors. The literature of the Geological Survey is steeped full of Croll's ideas, and especially of his ideas on the Ice Age, and few official geological books which have appeared since *Climate and Time* are unsophisticated by its teaching and conclusions. When my friend, the present head of the Geological Survey, the most fascinating and accomplished of living geologists, Sir Archibald Geikie, wrote the article " Geology " for the *Encyclopædia Britannica*, he transferred Croll's theory of an ice age bodily into his memoir, in which it occupies no inconsiderable place. He in fact allowed Croll to state his own case afresh in his own words in the article, and he described the theory in terms of hyperbolic praise. In his text-book of Geology (1893) he still accepts Croll's theory.

The dominance of Croll's views over the geological surveyors may be tested by another well-known handbook, namely, the sixth edition of Ramsay's *Physical Geology and Geography of Great Britain*, edited by Mr. H. B. Woodward,

F.R.S., where they are accepted without qualification or doubt, as if the geological world were of the same opinion. Another famous champion of the same views, Prof. James Geikie, the former director of the Geological Survey of Scotland, in his address to the Edinburgh Geological Society delivered so lately as November, 1891, on " Supposed Causes of the Glacial Period," takes his stand firmly on Croll's theory. He says : " That theory has been frequently criticised by physicists and others, to whose objections Croll made a final reply in his *Climate and Climatology*. In that work he has successfully defended his views and even added considerably to the strength of his general argument. I am not aware that since then any serious objections to Croll's theory have appeared. . . . At present, so far as I understand the facts, the glacial and inter-glacial phenomena are explained by the astronomical theory, and by no other " (*Edin. Geol. Soc.*, vol. vi., part iii., pp. 229, 230).

In 1894 Prof. Geikie brought out the third edition of *The Great Ice Age*. In this work Croll's theory, with Ball's supplement, still occupy twenty pages, and they continue to form the substantial foundation for Mr. Geikie's superstructure; but some doubts and hesitation are expressed as to their being quite competent to explain all the difficulties, and we are told of the various factors of the glacial problem as Mr. Geikie sees it, that " the primary cause of these remarkable changes is an extremely perplexing question, and it must be confessed that a complete solution of the problem has not been found." " Croll's theory," he adds, " has undoubtedly thrown a flood of light upon our difficulties, and it may be that some modification of his views will eventually clear up the mystery." A still more significant passage occurs in the preface, where Dr. Geikie, in explaining the removal of the discussion on the cause of a glacial climate to the end of the volume, says : " Its former position in the earlier part of the book had led some to believe that my explanation of the evidence was necessarily bound up with the late Dr. Croll's astronomical and physical theory. It will now be seen, I hope, that this is not the case. If I have read the geological evidence aright, the view supported by me will stand even should Dr. Croll's theory eventually

fail to find general acceptance." It would almost seem as if the facts and arguments adduced in *The Glacial Nightmare* and by Mr. Culverwell, which were both published between 1891 and 1894, had been more effective than the complete reticence of Mr. Geikie in regard to them might have led people to suppose. The ablest and most persistent of Croll's champions in America has been Mr. W. J. McGee, but I believe he stands almost if not quite alone there.

While it is true, therefore, that Croll's views and theories about an ice age have a rapidly diminishing support among geologists, they still maintain a certain hold in some influential quarters, and it becomes my duty to subject them to a closer analysis and to supplement what has been said about them in my previous volumes by some additional arguments.

If we could strip the earth of all its air and vapour, and treat it as if it were like the moon, we should find that it would receive nearly twice the amount of heat from the sun that now reaches it. Two-fifths of the heat coming to the earth from the sun is probably intercepted by its atmosphere. Tyndall has stated the conditions of an airless earth with his usual graphic force. He says : " The total amount of solar heat received by the earth in a year, if distributed uniformly over the earth's surface, would be sufficient to liquefy a layer of ice 100 feet thick and covering the whole earth. It would heat an ocean of fresh water 66 miles deep from the temperature of melting ice to the temperature of ebullition " *(Heat as a Mode of Motion*, pp. 476, 477). This, of course, implies that the heat so received would be retained and made to do dynamical work.

As a matter of fact only a very small quantity of it would be retained, the rest would be reflected back or be rapidly radiated into space again, and it has been calculated by Prof. S. P. Langley that the earth's temperature would become less than the freezing point of water even under a vertical sun. " What," he asks, " would be the temperature of the soil on a mountain-top rising wholly above the air, or what the temperature of the sunward hemisphere of the earth, if the present absorbing atmosphere were wholly withdrawn ? " To this he replies, that " in the absence of an atmosphere the

earth's temperature of insolation would at any rate fall below − 50° F., by which it is meant that (for instance) mercury would remain a solid under the vertical rays of a tropical sun were radiation into space wholly unchecked " (*Nature*, xxvi., p. 316). While,this would be the case in the hemisphere facing the sun, the unilluminated part would tend towards absolute zero. This then would represent what Humboldt called the astronomical climate, *i.e.*, the climate due entirely to the absorption and radiation of the sun's heat by the earth, apart entirely from its atmosphere. The fact can be tested by any one who will notice how at high elevations, where the air is very thin and tenuous, the temperature of the ground is very low, even under a vertical sun in summer. The moon is virtually such an airless globe with a purely astronomical climate.

It follows, on the other hand, that the climate of the earth is not a purely astronomical one, but is very largely dominated by meteorological considerations. The atmosphere is in a large measure the real dominator of the climatic problem. It acts in two ways : it intercepts a certain number of the solar rays, while it prevents the earth from radiating its heat into space at the rate it otherwise would do.

This is an elementary truth, but it is one that needs further sifting. Every substance apparently has a different capacity for absorbing radiant heat and for radiating it again. Solids as a rule have a much greater capacity than liquids, and liquids than gases. It would seem in fact conclusive from the experiments of Tyndall on the diathermancy of dry air (Preston, *op. cit.*, pp. 470-474), that perfectly homogeneous air containing no particles of a solid or liquid character is almost if not quite diathermatous, and allows radiant heat to pass to and fro through it without absorbing it to any but the slightest extent. If the atmosphere were therefore free from such particles, and especially if it were free from moisture, it would form a most indifferent blanket or perhaps no blanket at all to the earth's heat, and the radiation to and from the earth would go on as if it were an airless globe. The dry air would no doubt be heated by contact with the ground, and when so heated would carry off the heat so acquired by convection, but it would apparently not receive much, if any, additional

heat from radiation. If the earth were a homogeneous solid covered with an envelope formed of a perfect gas which was absolutely dry, what would happen would probably be as follows. In the first place, the ocean of air would be piled up somewhat as it is now.round the equatorial belt, partially by the centrifugal force which makes the earth spin round on its axis, and partly by the greater attractive force existing there in consequence of the bulging projection which forms a ring of thirteen miles deep round the waist of the earth. From the equator to the poles the depth of this aerial ocean would gradually diminish under the influence of these two causes. This piling up of the atmosphere is a condition of its equilibrium and must exist whatever the composition of the atmosphere, and we may put it aside for it does not touch our problem.

Let us turn to a more important matter. When we heat one portion of a gas more than another we naturally disturb its equilibrium. This can be restored either by conduction or by convection currents which carry the extra heat elsewhere, but much more effectually and largely by the gas expanding (if it be free to do so) in the direction of least resistance. Hence why a column of air heated by a fire rises in a chimney. Inasmuch as the equatorial region of the earth is a furnace on a great scale, the postulated dry air above it, when heated by contact with the surface of the ground, would, in the case we are discussing, rise and expand, but instead of rising perpendicularly, as in a chimney, the motion of the earth round its axis would make it rise at an angle to the surface, and in the contrary direction to the earth's motion.

Again, it is clear that, when free to rise, a column of dry heated air is higher and lighter than a precisely equivalent column of unheated air; it is lighter because it is as a whole further from the earth's centre to which gravitation tends. The consequence is that, in order to restore equilibrium between two adjoining columns of air at different pressures, a portion of the air of the taller column would flow over at the top of the shorter one, while part of the heavier column next to it would flow in at the bottom of the lighter one, and thus the tension would be momentarily relieved. This is the elementary *modus operandi* of the circulation of air as seen in

every household fire. It is also the explanation of nearly all winds, which are simply the efforts of nature to restore equilibrium between areas of high and low pressure. The earth differs from an area heated by an ordinary furnace in that its heat is not concentrated in one place, but it is continuously heated in a diminishing degree from the equator to the poles, and the relative pressure of a perfectly dry mass of air covering it would, if the earth were a homogeneous solid, gradually diminish with the increase of latitude. The problem of equilibrium under these conditions is not quite simple, and it has not always been understood. It would seem that under such conditions every particle of air in contact with a part of the earth's surface warmer than itself would be pulled at by two forces, one tending to make it rise perpendicularly, the other dragging it laterally to supply the loss of tension caused in a neighbouring area by the air over it being expanded and rising. Which of these two forces would prevail in any particular case would depend on their relative potency. Again, in regard to the return currents in the upper air which would go to complete the circulation. These currents, if the air were absolutely dry, would in their journey north and south lose considerable heat by radiation and convection, and this cooling would condense them and make the air in them heavier and cause it to sink, just as the Anti-trade winds do now.

When they reached the sea level or the ground, these cooled dry heavy winds, following the analogy of the Anti-trades, would turn round and complete the circle of air-current circulation, while the dry air heated by the ground, but not so much so as in more tropical latitudes, relieved from having to rush laterally to restore the local tension, would on the contrary rise, as we have postulated the air in the hottest zone would rise. This is not mere hypothesis but a serious argument, and it is one overlooked by Croll and others, who continually argue as if but for local circumstances it is perfectly clear that the general circulation of the atmosphere would be *and is now* between the *equator* and the *poles* which are so far apart. Every analogy seems to me to concur in the view that if we reduce the problem to the very simple one of a perfectly dry atmosphere

over a perfectly homogeneous solid globe, there would not be one system exchanging hot and cold air between the equator and the poles, but two or several systems of circulation, and there would be uniform and persistent climatic zones girdling the earth, each of them with an air-circulation of its own, which zones would travel bodily north and south with the progress of the sun from one tropic to the other and *vice versâ*, but be otherwise constant.

Having considered a hypothetical waterless earth with a dry atmosphere, let us now shortly consider an equally hypothetical one covered with a uniform mantle of water.

The difference between the land and the sea in regard to absorption is very notable. While the former only absorbs heat directly on its surface, and the ground below the surface only gets any heat it derives from the sun by conduction, in the case of water, which is transparent to the sun's rays, it is not merely its surface layers which absorb the sun's heat, but layers lower down.

Buchan points out that in the case of water the radiant heat reaching it " is very far from being arrested at the surface, as in the case of land. It rather penetrates to a considerable depth. The depth to which the influence of the sun is felt has been shown by the observations made during the cruise of the *Challenger* to be, roughly speaking, about 500 feet below the surface of the sea. The rate at which in perfectly clear water this heat is distributed at different depths is a problem that has not yet been worked out. Since water is a bad conductor, the heat thus distributed does not, as takes place with respect to land, penetrate to still greater depths by conduction, but only by different densities prevailing at the same depths, whether these different densities be due to different temperatures or different degrees of salinity. Thus one of the more important distinctions between land and water surfaces in their bearing on climate is that nearly all the sun's heat falling on land is arrested on the surface, whereas in water it is at once diffused downwards to great depths " (*Encyclopædia Britannica*, xvi., p. 116).

Water, while not a ready absorber, is a great deal more stubborn than the land in retaining any heat which it absorbs, and radiates it much less readily.

Having thus discriminated their different methods of absorbing heat, let us also discriminate between what becomes of the heat actually absorbed by the land and the water respectively. In the case of the land, as we have seen, this heat when not radiated into the air remains in the locality where it was first received except such as passes into lower strata or adjacent areas by conduction, and this for the very good reason that the ground is stationary and not mobile.

In the case of the water, as in that of the air, the result is entirely different. Directly the water becomes warmer by absorption of heat it gets lighter and begins to move away and is replaced by cooler water, and, instead of remaining to warm the locality, any heat thus falling on it is carried off into other areas by the water itself, and as it parts with the heat absorbed very stubbornly, it can carry the heat thus received for long distances and affect the climate of far-off places.

The water thus moved is partly under the influence of convection currents, which cause a local circulation among its various layers, but much more largely it moves under external impulse. Its upper layers are largely under the influence of winds blowing over it, and are driven along by such winds in continuous and corresponding currents. These currents carry heat, as we shall see presently, in very great quantities from the warmer latitudes to the colder ones, and thus temper the climate of the latter.

The direction and force of these currents depend almost entirely, as Croll has shown, on the winds, and the winds depend, as we have seen, on the distribution of the areas of high and low barometrical pressure.

It is quite plain that since the equatorial region became the great furnace of the world, so long must it also have been a great belt of low pressure bounded on each side by winds blowing from the north-east and south-east respectively. This must have been the case even if the earth were ever either a waterless globe or one entirely covered with water. Hence if the earth were entirely covered with water, there must have been a great and constant equatorial current running from east to west, the resultant of the winds in question, and running continuously round the world in the same direction. This would be matched by other similar

currents running parallel with it and in the same direction in the higher latitudes, probably separated by currents running in the opposite direction, caused by the returning winds or Anti-trades; but, so far as we can see, there would be no ocean currents at all induced by winds running directly towards either pole from the direction of the equator, or *vice versâ.*

So much for the movements of the water in a globe entirely covered with water; now for the air movements in a similar globe. While the balance between the heat received and the heat radiated or reflected by the land either goes to warm its surface or is propagated downwards and laterally through the soil, a large portion of that which falls on the water does not tend to raise its temperature at all, nor is it carried away by convection currents or by conduction. It goes on the contrary to convert a large quantity of it into vapour, in which process of evaporation it becomes dormant or latent and ceases to be sensible, and it only becomes sensible again (and in the same proportion) when the vapour is condensed again into water. Let us now see what happens when the air becomes charged with the vapour of water. It is a well-known property of gases that the fact of any space being occupied by a gas does not at all affect the capacity of the space for being invaded by any other gas. Thus a vessel filled with hydrogen, so that it cannot take in any more, acts exactly like an empty vessel towards any other gas. Dalton, Gay Lussac, and Regnault proved that the same holds good with vapour. Hence the atmosphere can absorb vapour to the full extent of saturation, just as if the evaporation of the latter had taken place in a vacuum. Dalton also showed that by adding watery vapour to air its weight with the same tension instead of being increased is diminished. Thus a column of damp air weighs less than a column of dry air with precisely equivalent tension; that is why air charged with vapour makes the barometer fall.

Thus when a column of air is not only being heated but is also being continually recruited by moisture, it has a double tendency to rise into the upper air, and when it has risen a certain distance it expands, radiates heat, and cools, and in consequence forms clouds.

Let us now turn to a very polemical subject, and one in which opinions still greatly differ, namely, in regard to the effect of vapour in the air upon radiation of heat. Tyndall, who is followed by most meteorologists (and as I think with good reason in this case), holds that the really effective blanket which maintains the temperature of the earth at its high level, and which also largely absorbs heat itself, is the vapour in the air. As Mr. Becker says: "Moisture affects absorption of heat, greatly increasing it, so that relatively moist air tends to become hotter, while dry air tends to sink below the average temperature". It was one of Tyndall's great services to physical science to first urge the extent of this action of vapour in the air. In regard to it he says: "Had we not been already acquainted with the action of almost infinitesimal quantities of matter on radiant heat, we might well despair of being able to establish a measurable action on the part of the aqueous vapour of our atmosphere". This vapour forms only about $4\frac{1}{2}$ per cent. of the atmosphere. Tyndall claimed to show by laboratory experiments that the aqueous vapour contained in the atmosphere of the room in which he experimented exerted an action on radiant heat seventy-two times as powerful as that of the air itself (*Heat as a Mode of Motion*, p. 351). He afterwards tried the experiment, he tells us, with air from Hyde Park, Primrose Hill, Hampstead Heath, Epsom Downs, Newport (Isle of Wight), and the sea beach near Black Gang Chine, and in each case the aqueous vapour of the air from all these localities, examined in the usual way, exerted an absorption seventy times that of the air in which the vapour was diffused (*ibid.*, p. 358).

His view of the effect of this is clearly stated by himself. "The withdrawal," he says, "of the sun from any region over which the atmosphere is dry must be followed by quick refrigeration. The moon would be rendered entirely uninhabitable by beings like ourselves through the operation of this single cause. With a radiation uninterrupted by aqueous vapour, the difference between her monthly maxima and minima must be enormous. The winters of Tibet are almost unendurable from the same cause. . . . The refrigeration at night is extreme when the air is dry. The removal

for a single summer night of the aqueous vapour from the atmosphere which covers England would be attended by the destruction of every plant which a freezing temperature would kill. In the Sahara, where the soil is fire and the wind is flame, the cold at night is painful to bear. Ice has been formed in this region at night. In Australia also the *diurnal range* of temperature is very great, amounting commonly to between 40° and 50° " (*ibid.*, p. 366).

It is the vapour in the air which, according to Tyndall, prevents these extremes of temperature and these aberrations from a common and endurable mean, and he urges as quite clear that not only would an airless globe be unendurable, but a vapourless atmosphere would be so also.

This, then, is the view maintained by Tyndall. Magnus, on the other hand, an experimenter like himself of the first rank, has proved the desperate difficulty of conducting conclusive experiments on a subject where so many interfering causes intervene, by arriving at conclusions very different to those of Tyndall. He holds that dry air is by no means so diathermatous as he makes out, and that it absorbs a considerable quantity of heat, while damp air varies but little from it.

The experiments of Tyndall and their results were contested, as I have said, by Magnus, who came to precisely the opposite conclusions to his in regard to the diathermancy of dry and moist air respectively. The former he put considerably higher than Tyndall had done, while he found that there was little difference between them instead of their being markedly different. More lately Lecher and Peruter (*Phil. Mag.*, January, 1881) have tried experiments, and their conclusions were at issue with both inquirers, since they found that both dry and moist air are alike very diathermatous, and thus the matter stands.

I am bound to say that Tyndall's skill as an experimenter, and the precautions he invariably took, give to people like myself a great faith in his results in issues of this kind. Besides which his results seem to be confirmed by *a priori* considerations. Thus there can be no doubt about the opacity of *water* to radiant heat. It stands at the very top of the list of liquids in this respect (Preston, *Heat*, p. 478) ; *a priori*,

therefore, we should expect that aqueous vapour would also prove itself highly opaque to radiant heat, and this is supported by the experiments tried upon other liquids than water. Thus as Preston, whose own conclusion leads him apparently to support Tyndall, says : " It appears that in the main the molecules maintain their characteristics as absorbers of radiant heat although the state of aggregation changes, and if any inference be allowed, we should expect that aqueous vapour would be exceedingly opaque to thermal radiation, for, as we have already seen, pure water stands at the bottom of the list as a transmitter of radiant heat " (*ibid.*, p. 479).

While Tyndall's view, therefore, seems right, and I do not hesitate to follow it, he has probably somewhat exaggerated the blanketing effect of vapour in a very gaseous state, and somewhat minimised its real great potency when it is condensed into fog or cloud. As Woeikof has pointed out, it is when it is condensed in small ice crystals or water droplets, when it forms mists and clouds (even when they are perfectly transparent to light and invisible to our eyes), that vapour becomes a really efficient screen, just like dust and smoke do. It is the humidity of the air rather than the amount of gaseous vapour it contains which is the true measure of its screening effect. This is however a secondary issue ; what it is important to remember is that water held in suspension in the air in the form of visible or invisible clouds is a most efficient instrument in blanketing the earth's temperature.

It follows from this, as Prof. Langley says, that " the temperature of a planet may, and not improbably does, depend far less upon its neighbourhood to or remoteness from the sun than upon the constitution of its gaseous envelope, and indeed it is hardly too much to say that we might approximately indicate already the constitution of an atmosphere which would make Mercury a colder planet than the earth, or Neptune as warm and habitable a one.[1] . . . Remember-

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[1] I may add here, although it is not germane to my subject, that Prof. Langley has shown that without our atmosphere the sun would appear of a strongly bluish tint. This would be the appearance of the sun to anyone viewing it from space. Its present colour is due to the fact that the air absorbs the short waves of light more readily than the long ones.

ing, then, that it is not merely by the absorption of the air, but by the selective quality of the atmosphere, that the actual surface temperature of our planet is maintained, we see that without this comparatively little known function it appears doubtful whether, even though the air supported respiration and combustion as now, life could be maintained on our planet " (*Nature*, xxvi., p. 316).

The fact of the air not being dry, but being charged with vapour, thus makes a great difference to climate. The heat in damp air, as we have seen, is there in two forms : in the form of sensible heat, which raises the actual temperature of the air, and in that of latent heat, which has been spent in the work of evaporation and is retained by the vapour. As the vapour is lighter than air, it follows that damp air with the same amount of sensible heat in it will rise faster and travel farther upwards than dry air. As it rises from any hot region cooler and drier air will come in from elsewhere to take its place, and a similar circulation to that before described will ensue, with one important distinction, namely, that when the damp air reaches a sufficiently cold zone it will not only gradually part with its sensible heat, but its vapour will become condensed again into water, which will fall in the shape of rain. This condensation will release its latent heat and convert it into sensible heat, so that the fall of rain, especially in great quantities, will have the effect of again heating and expanding the air which has been undergoing the process of cooling and condensation. This is why showers of rain and falls of snow always warm the air. Let us now apply these conclusions to the hypothetical case of a world entirely covered with water. There would in such a case be an enormous evaporation in the equatorial belt, and the vapour in the air there would be immensely recruited by that brought in by the north-east and south-east winds, answering to the present Trades. This very light and damp mass of air would rise very rapidly, just as it does now, over the tropical ocean areas, and presently having got expanded and cooled sufficiently, would discharge enormous deluges of rain, as it does now, in the tropics, only on a larger scale, since there would be nothing but water for the sun to beat upon ; after which the air, which would be still further heated by the process,

having lost its moisture, would behave like that from a water-less globe, rise higher, then travel north-east and south-east, and presently getting cooled and becoming heavier sink again and join in the general circulation as I have described. While the weather would be constant in this equatorial zone all round the world, north and south of it there would be other zones in which the same phenomena would be repeated on another scale, since the evaporating power of the sun would naturally be much less in the higher latitudes.

Having thus considered what would happen in a waterless globe, and in one covered with water, let us now turn to the actual state of things in which we have a mingling of the two conditions, three-fourths of the earth's surface being covered with water and one-fourth being dry land. If the land and water were arranged symmetrically on the earth, as they are in the planet Mars, the climate of the earth would be very regular and simple, but as land and water are distributed most irregularly on our globe, it confuses the problem greatly. It is plain, however, in a general way, that the conditions pre-vailing over the actual land resemble hypothetical conditions on a waterless globe, and those existing over the actual sea resemble those which would exist on the water-covered earth we have been discussing.

The presence of Central America, Africa, etc., right across the equatorial regions, prevents the equatorial current from going round and round the world, as it would inevitably flow if it were free to do so, and diverts it at two important points at least, from the torrid zone towards higher latitudes (namely, in the case of the Gulf Stream and the Kuro Sivo current), greatly affecting their climate, and so long as these barriers existed and exist, so long must the warm equatorial current have been diverted from its normal course. In regard to the circulation of the air, the fact that the Trade winds have an outer limit and boundary as well as an inner one, where they impinge on the region of calms, is a good proof of what I have always contended for, that the region of calms and those of the two Trades form a separate meteorological pro-vince altogether, with a separate circulation of winds and of weather, from the temperate and arctic zones to the north and south of it respectively, and that we have no warrant for

the popular view thus expressed by Balfour Stewart: " The polar regions being manifestly colder than those of the equator, we have convection currents of hot air passing in the higher atmospheric regions from the equator to the poles, and the current of cold air passing in the lower atmospheric regions from the poles to the equator. These latter are known as the Trade winds, and the former as Anti-trades."

It is clear to me that if we are to deal adequately with any meteorological explanation of an ice age in the higher latitudes, we must exclude the area within the two tropics from our view, and limit ourselves, at all events in regard to atmospheric phenomena, to the temperate and higher zones. We must not appeal, either, to the vapour made in the great equatorial boiler to supply us with materials for huge snow sheets and ice sheets further north and south. That equatorial vapour must always, as it does now, have discharged itself in the shape of tremendous tropical rains. If it be absolutely necessary for the argument of Croll and his followers that we should have a highly heated boiler as well as a very good condenser, we cannot plant the former in the tropics. The steam and vapour raised by the tropical sun from tropical waters must always, so far as we can see, have returned as it does now to the sea and the land in the tropical belt itself, the great region of heavy rains and of a depressed barometer.

On the other hand, the warm tropical winds which rose from the land, and the warm damp winds which came originally from the sea-latitudes of the tropics, must have had their moisture squeezed out of them as they have now, and must also have afterwards got very speedily cooled, first by radiation which increases, according to Stefan's law, as we have seen, enormously and in proportion to the fourth power of the absolute temperature of the radiating body, and partly by convection, etc., and have sunk again to the ground as cool Anti-trades, just as they do now; the Northern Trades coming down perhaps not very far north of the tropics in the latitudes of Ascension and Teneriffe, as we still find them doing.

I do not dispute the postulate that climate is greatly affected by water and air currents, especially by the former. I only protest against an hypothesis, which recurs regularly as a shibboleth in geological literature, namely that the tropics

and the temperate or polar region have a common circulation of air currents. This has been the view not merely of Croll and his champions, but also of his opponents. Notably of a very ingenious critic of Croll's, one who, working with Croll's postulates, has come to a more rational conclusion than Croll, but who has at the same time taken his stand on the impossible premise that, in regard to air currents, the equator and the poles are the boiler and condensers which now dominate climate, and which dominated it still more in glacial times. I mean Mr. Becker.

Croll's fundamental principle, as it will be remembered, is that the glacial age took place in a time of great eccentricity with winter in aphelion. His acute critic, Mr. Becker, has entirely questioned the force of this his prime postulate, and proclaimed that the most efficient conditions to produce an ice age are the very reverse of those demanded by Croll. In August, 1894, he published a paper in the *American Journal of Science* on certain astronomical conditions favourable to glaciation. In this he does not calculate the quantitative effects of the conditions which he discusses. He has rather discussed the tendency of certain causes. His main conclusion is that, instead of extreme eccentricity of the earth's orbit being the most favourable to glacial conditions, it is the least favourable, and that the most favourable is the period of least eccentricity.

" The conditions present in a glaciated area," he says, " should be that the torrid and lower temperate zone in which evaporation chiefly takes place should be as warm as is consistent with other conditions, for it must be remembered the tension of aqueous vapour increases much more rapidly than temperature, and so also must the rate of evaporation. On the other hand, the cold in high latitudes must be great to promote condensation in the form of snow ; besides which the temperature gradient should be high or steep, because the energy available for wind, and for water currents due to winds, is in direct proportion to the difference of temperature. The great foe to glaciation in summer is rather warm rain than sunshine, for warm rain represents heat transferred from lower latitudes to higher ones. A certain amount of sunshine in high latitudes will not seriously diminish the accumulation of névé; for a great part of the winter snowfall has a tem-

perature far below freezing ; and in summer, water resulting from superficial melting will freeze again as it percolates through subjacent snow until the entire accumulation of the past winter is raised to the melting point. Such a process is apparently essential to the formation of glacier ice. While a portion of the direct sunshine is harmlessly employed in converting snow to ice, another and very large part will be reflected from the névé fields. Hence it seems," says Mr. Becker, " that the features of a summer climate in a glaciated hemisphere which are most favourable to ice accumulation are cool tropics and a low temperature gradient towards the pole, even if the sunshine in very high latitudes must be increased to bring about a dry climate."

The winter specially favoured by Dr. Croll for setting his machinery going was the long winter in aphelion when the earth's eccentricity was the greatest. This winter would of course be a cold winter as compared with that of zero eccentricity, *and the difference would be most marked in the tropics,* for an increase in the length of the winter diminishes all heat rates or temperatures in the same proportion, and since the rates are highest in the tropics the greatest decrease must also take place in the torrid belt. The indications of the table Mr. Becker quotes show that the temperature of *January* would then be the greatest at the Tropic of Capricorn, and that it would be no warmer there then than it is now in *July* at 45° of north latitude.

The result of this drop in the winter temperature of the tropics and adjoining regions would be, he argues, that there would be a corresponding drop in the evaporation, and the consequent snowfall in high latitudes would be small. The heat gradient during these conditions would also be less steep than it is possible to make it by any other combination of conditions, and therefore the winter would be the calmest, driest, and coldest possible.

On the other hand, the summer in the same hemisphere would be the hottest possible. The evaporation would be correspondingly enhanced. The heat gradient towards the pole would be considerably greater than it is now, or than it would be at the time of zero eccentricity. Hence the summer would be wet as well as hot, and the reduced snowfall of the

winter would have to face not only a fiercer direct summer sun, but heavier warm rains and stronger warm winds, both great snow eaters. In addition, a good deal of what now falls as snow in high latitudes in late spring and early autumn would then fall as rain. It seems to me impossible to resist Mr. Becker's conclusion that the period of greatest eccentricity was in fact the period most unfavourable to glaciation, the snowfall being the smallest and the summer rainfall the largest which can occur with the present obliquity.

On the other hand, with zero eccentricity the seasons would be of equal length and the climate would be intermediate between those of the present time in the two hemispheres. The winter would be a little cooler than now throughout the northern hemisphere, and the temperature gradient a little smaller; other things being the same the winter precipitation would be somewhat smaller, but more of it would fall in snow. In summer the July temperature would be two or three degrees Fahrenheit higher than it is now, and the heat gradient would almost imperceptibly exceed the present, the winter would be what is now considered a cold one, and the summer such as is now thought unusually warm in our hemisphere—that is to say, the conditions for glaciation would be considerably greater than in those postulated by Croll.

A zero eccentricity is a mere possibility, and the present eccentricity is almost at the nearest point to zero that the eccentricity has reached for a very long period, so that, if Mr. Becker's analysis be correct, we have at this moment in the northern hemisphere a condition of things more favourable to glaciation than occurred at the time of the greatest eccentricity, or in fact at any other time ; and if a glacial period were possible under such combined astronomical and meteorological conditions as were postulated by Croll, this is the very time when it should be operating in our hemisphere, and, as we shall see presently, this view is corroborated by what is now taking place in the planet Mars.

Let us now turn to Croll's own more concrete case.

Croll was a rigid uniformitarian. He faced the problem of explaining the glacial age without calling in any violent or other changes to supplement the ordinary and current operations of nature. He appealed to no transcendental astro-

nomical conditions and to no geographical changes, to no
great upheavals or subsidences, but he claimed to find an
explanation of his theory in accordance with the most rigid
conditions of uniformity.

Croll's researches were devoted to two points: first, to
the discussion of the effects of certain meteorological changes
induced by a high eccentricity of the earth's orbit with winter
in aphelion, from which he concluded that this cause was com-
petent to create an ice age; and secondly, to the discussion of
the cause of the transfer of glacial conditions from one hemi-
sphere to the other with a corresponding alternation of mild
periods, which he claimed as a condition of his glacial period.
Before I discuss these issues I will first call attention to
two matters, not of argument but of fact, in which Croll
was clearly mistaken.

In answering some of his critics in a paper published in
the *Philosophical Magazine* for October, 1883, Croll tells us
that one of the most important factors in the theory of
geological climate resulting from changes in the eccentricity
of the earth's orbit is obviously the temperature of stellar
space. Unless we have, at least, some rough idea of the
proportion which the heat derived from the stars bears to
that derived from the sun, we cannot form any estimate of
how much the temperature of our earth would be lowered or
raised by a given decrease or increase of the sun's distance.
He then goes on to say that Pouillet and Herschel had cal-
culated by different methods that the temperature of space
is − 239° F., or 222° higher than absolute zero, that is, than the
temperature a body in space would acquire if neither the sun
nor the stars existed. As the mean absolute temperature of
the earth is about 521°, he accordingly concludes that the
proportion of heat received from the stars is to that received
from the sun as 222 to 299 (*op. cit.*, pp. 241, 242). No doubt
Dr. Croll suggests that in his own opinion this proportion is
altogether untrustworthy, and that the temperature of space
is not far from absolute zero, but nevertheless he tells us
that his determinations of the change of temperature due to
changes in the sun's distance were computed on these data,
and Newcomb was justified in his reply in saying that the
data in question have no sound basis. The latter goes on to
say: " Photometry shows that the combined light from all

the stars visible in the most powerful telescope is not a millionth of that received from the sun, and there is no reason for believing that the ratio of light to heat is incomparably different in the two cases ".

Let us now turn to a second fallacious datum of Croll, which was pointed out by Culverwell.

Following an assumption of Herschel, he calculated that the radiation of heat from the earth is proportionate to its absolute temperature, but it has been known for some time that this is not the true law, and in 1879, as we have seen, Stefan's law was published which showed that the radiation increases as the fourth power of the absolute temperature (*Nature*, vol. xx., p. 89). This could not, of course, have been known to Croll in 1875, when the first edition of his *Climate and Time* was published, but it was known and published several years before the second edition came out. Let us now turn to some of Croll's meteorological arguments.

A quite disproportionate part of his *magnum opus*, his *Climate and Time*, is taken up with establishing what is not now in much dispute, if disputed at all, namely, the potency of ocean currents and of air currents in the modification of local climates. Within certain limits, such as those I have already discussed elsewhere at great length, it is clear that a considerable part of the heat which falls upon the ocean in warm climates is carried away to other places either by water currents or air currents, and similarly with the heat absorbed by the drier air over the land. The warm air travels north and south from the lower hot latitudes to the higher cool ones, and tends to deprive the former of a portion of the heat they have absorbed and to warm the latter. Hence it is clear, as Croll urges, and the fact is so elementary that it is everywhere conceded, that the climate of every part of the earth is other than an astronomical climate pure and simple, due merely to the sun-heat it receives. The amount of heat received by any place from the sun is only in fact one element in fixing its climate. That climate is sophisticated and altered very largely by the carriage of heat from one place to another by currents of water and currents of air, and this is especially the case in the higher latitudes. No one has done more to emphasise and illustrate this great truth than Croll himself.

Limiting himself to the Gulf Stream, Croll has calculated that nearly one half as much heat is transferred from tropical regions by the Gulf Stream as is received from the sun by the entire arctic regions, the proportions being nearly as two to five. This is calculating the problem as if the sun's rays were direct instead of being oblique in the arctic regions, and being thus less largely absorbed by the air than now. If this fact is also taken into consideration, it would appear that the Gulf Stream really supplies the arctic regions with half as much heat as they receive from the sun. Mayer similarly says of the effect of the great Kuro Sivo current in modifying the climate of America, that the mean temperature of Sitka is higher than that of St. Louis, though the former is more than 18° farther north than the latter, and he adds : " The effectiveness of these and other vehicles of warmth and moisture was probably never much greater than to-day. Hence if now, under the present favourable conditions, the arctic regions do not receive enough moisture to form a continuous polar ice field it may reasonably be doubted if they ever did so."

This convection of heat is very much more potent and effective in so far as it is carried by currents of water than by currents of air.

If we compare the amount of heat conveyed by the Gulf Stream with that conveyed by aerial currents, the result, as Croll says, is startling. The density of air is to that of water as 1 to 770, and its specific heat is to that of water as 1 to 4·2 ; consequently the same amount of heat that would raise 1 cubic foot of water 1° would raise 770 cubic feet of air 4·2°, or 3,234 cubic feet 1°. The quantity of heat therefore conveyed by the Gulf Stream is equal to that which would be conveyed by a current of air 3,234 times the volume of the Gulf Stream at the same temperature and moving at the same velocity. Taking the width of the stream at 50 miles, its depth at 1,000 feet, and its velocity at two miles an hour, it follows that in order to convey an equal amount of heat from the tropics by means of an aerial current it would be necessary to have a current about one and a quarter mile deep and at the temperature of 65° blowing at the rate of two miles an hour from every part of the equator over the northern hemisphere towards the pole. . . . A greater quantity of heat

is probably conveyed by the Gulf Stream alone from the tropical to the temperate and arctic regions than by all the aerial currents which flow from the equator " (*Climate and Time*, pp. 27, 28). I do not propose to quote further from Croll on this issue. His conclusion in regard to the vast influence on climate of the heat transported by water and aerial currents seems incontrovertible. It is curious, however, that the most important foster-father of this perfectly sound view should have apparently entirely forgotten or overlooked it when he was calculating and enlarging upon the mediate effects of a varying eccentricity of the earth's orbit upon terrestrial climate. In dealing with a large part of the problem, he argues throughout as if the only thing he had to consider was the effect of direct sunshine upon the ice and snow. Croll begins by arguing that in the absence of the sun the temperature of the earth would fall to the temperature of space, and for this, as we have seen, he accepts Pouillet's figures, namely, – 239° F. He argues further that in England, for instance, the mean midwinter temperature is kept at 40°, that is to say, 280° F. above that of space, *by the sun heat alone;* and having calculated that the midwinter intensity of sun-heat in the time of greatest eccentricity will be reduced 16 per cent., he urges that the 280° of temperature above mentioned will be reduced by a proportionate amount, namely, by 45° F.; a very substantial figure indeed. But, as Mr. Culverwell says, this argument is fallacious, because Croll ought in the first instance to subtract from the 280° just mentioned all that portion of the midwinter heat above named which *is due to ocean currents,* and then to make his calculation.

If we correct Croll's figures in the first place by substituting Stefan's law for the erroneous law of radiation which he employs, and secondly by taking into consideration not only the sun-heat received by high latitudes, but also the convection of heat thither by ocean and air currents, we shall have to greatly modify his result.

The substitution of Stefan's law compels us to reduce Croll's critical figures from 45° F. to 11° F., while, as Mr. Culverwell adds, if we further consider that ocean and air currents are twice as effective as winter sun-heat in main-

taining the temperature, we should get a lowering of the midwinter temperature by about 4° F. only (Culverwell, *Phil. Mag.*, 5th series, No. 38, p. 552).

This shows how very misleading Croll's figures and methods are. Culverwell tests the position by an actual case, and takes that of Yakutsk in Siberia, which is supposed to have the most severe of terrestrial climates. " The excess of its summer temperature over $-239°$ F. is about 309° F., and of its winter temperature about 199° F., but the midsummer sun-heat is to the midwinter sun-heat as 5,800 is to 199, or 309 to 11." If Croll's theory be tenable, then, since according to him the excess of midwinter temperature over $-239°$ F. is due to the midwinter sun-heat alone, the midsummer temperature ought to be tested by the excess of sun-heat alone, and to be 5,800° F. above $-239°$ F., which is of course a fantastic and ridiculous position to maintain. " On the other hand, if the summer excess of 309° F. is due to summer sun-heat alone, then the midwinter temperature ought, if only dependent on sun-heat, to be only 11° F. over the $-239°$ F., *i.e.*, it ought to be $-228°$ F., and the rest of the 199° excess over the $-239°$ F. must be due to heat derived from other regions of the earth." This shows in a forcible way how utterly wrong Croll has been in overlooking the very factor he elsewhere lays most stress upon, namely, convection of heat, when he is testing the problem by a concrete case.

Sir Robert Ball accepts Croll's method of dealing with this part of the question, and like him ignores in his calculation that proportion of the heat of any latitude which is derived from other latitudes by convection, and he also ignores Stefan's law, which had been published ten years when he wrote his book. He differs from Croll in two ways only, namely, in taking the temperature of space at $-300°$ F. instead of $-239°$ F. He also bases his calculation as to change of climate upon *the daily average* of sun-heat received in winter, and not upon the amount of heat received on *midsummer day* as Croll did, which is no doubt a safer and better plan, since, as Culverwell says, the adjustment of temperature to sun-heat can hardly be instantaneous. Ball, calculating the problem according to his method, finds that in the glacial winter there would be a deficiency of 7 per cent. in the average daily supply

of sun-heat as compared with our present winter supply, the unit being the average annual daily sun-heat; and inasmuch as he argues that this unit keeps Great Britain 353° above the temperature of space, it follows, although Ball does not himself publish the calculation, that that calculation makes the lowering of the winter temperature of Great Britain to be 24·5° F. instead of the 45·3° of Croll (Culverwell, *Geological Magazine*, 1895, p. 61); but, as we have seen in the case of Croll, the whole basis of this calculation is fallacious, because Ball, like Croll, ignores Stefan's law.

Mr. Culverwell again puts a concrete case to show how fallacious Ball's argument is in this behalf. Taking Great Britain, the ratio of the daily sun-heat in summer to that in winter is about 1,000 to 199, or 309 to 62. Hence, if the winter temperature be due to the sun-heat, the summer temperature should be 1,000° F. over − 300° (this being Ball's zero), or, say, 700° above the zero of the Fahrenheit scale; or if the summer temperature be that due to sun-heat alone, then the winter temperature ought to be − 300° + 62° F., or − 238°, instead of about − 40°.

Again, Culverwell shows that Croll's statement, "that owing to the greater distance of the sun in the long winter of greatest eccentricity much less daily sun-heat would be received in the long winter on any latitude than is now received in an equal time on the same latitude," is vitiated by the fact that the law in question would only apply to those regions on the earth where the sun actually shines, so that it would be scarcely felt in the arctic regions, and be chiefly felt in the southern tropics, where the heat received by solar radiation being greatest, the percentage decrease is also greatest. In fact, the only difference at the North Pole would be that while it is now only 179 days without any sun-heat, it would in the time of greatest eccentricity be 199 days in the same condition; but since the lowest temperature would be in January, it seems unlikely that the slightly longer absence of the sun would make much difference (*ibid.*, p. 6).

In this behalf Seebohm has produced some graphic examples from his Siberian experiences, in which not merely insular patches of snow have disappeared like shadows, but it has been cleared off vast continental areas in a very short time.

I cannot resist the temptation of quoting one effective passage from his address to the British Association describing the effect of an incoming Siberian summer. He says, speaking of the basin of the Yenisei :—

"We afterwards learnt that during the last ten days of May a tremendous batfle had been raging 600 miles as the crow flies to the southward of our position on the arctic circle. Summer, in league with the sun, had been fighting winter and the north wind all along the line, and had been hopelessly beaten everywhere, as 'we were witnesses that it had been in our part of the river. At length, when the final victory of summer looked the most hopeless, a change was made in the command of the forces. Summer entered into an alliance with the south wind. The sun retired in dudgeon to his tent behind the clouds, mists obscured the landscape, a soft south wind played gently on the snow, which melted under its all-powerful influence like butter upon hot toast; the tide of battle was suddenly turned, the armies of winter soon vanished into their water and beat a hasty retreat towards the pole. The effect on the river was magical. Its thick armour of ice cracked with a loud noise, like the rattling of thunder; every twenty-four hours it was lifted up a fathom above its former level, broken up first into ice floes and then into pack ice, and washed down the stream at least a hundred miles (in the twenty-four hours). Even at this great speed it was more than a fortnight before the last struggling ice blocks passed our post of observation on the arctic circle, but during that time the sun had risen seventy feet above its water level, although it was three miles wide; and we were in the middle of a blazing hot summer, picking flowers of a hundred different kinds." As late as 1st June the snow was six feet deep at Seebohm's post of observation and averaged five feet over the million and more square miles which are drained by the Yenissei, an area four times that drained by the Danube : so suddenly did the south wind melt an area of 1,000,000 square miles, covered with snow five feet deep.

This picture may be placed by the side of those I have given on pages 388 and 389 of my *Glacial Nightmare.*

Let us now try and come closer face to face with Croll's

position. One of the difficulties of so facing it, to which I called attention in my former work, is that in his voluminous writings he devotes such a very small space to it himself. The greater part of his writings which professedly deal with his theory of the glacial period are occupied with largely irrelevant polemics in regard to the cause of ocean currents, etc., on which issues he is apparently generally right, and has no real opponent. It is in the main from his answers to his critics that we have to collect an account of what his theory really is.

He tells us then that, while he utterly discards any astronomical cause as completely insufficient by itself to produce a glacial period, he holds that an extreme eccentricity of the orbit combined with winter in aphelion will set in motion a number of mediate meteorological and other physical forces which are competent to initiate such a period. This he repeats frequently. When we try and discover, however, what are the particular agencies he means, we have some difficulty in finding them. The most specific statement he makes is that the astronomical conditions just named would " cause the winter to be much longer and colder than at present. Snow in temperate regions would then fall in place of rain, and although the snowfall during the winter might not be great, yet, as the temperature would be far below the freezing point, what fell would not melt." All these are brave statements, but as they stand they are mere *obiter dicta*, and ought to be proved. Unfortunately no proof of them is forthcoming or, as it seems to me, possible. As I have already shown, it is a mistake to suppose that much, if any, of the vapour which is made in the equatorial zone ever reaches high latitudes at all. It nearly all returns to the sea again in the deluging rains of the tropics. Again, it must be remembered that Croll cuts off a vast proportion of the vapour now being formed in the temperate regions by directing the Gulf Stream to the other hemisphere during all the so-called glacial age. The Gulf Stream is the great mother of our rain-fall. Hence, if we are to have a heavy snowfall in high latitudes, which means a low temperature in the temperate regions in winter, where is the vapour to come from to be condensed into snow ?

5

The damp winds that feed the present snowfall in the highest latitudes come not from the equatorial zone but from the temperate one, and if we make the temperate zone colder we paralyse the evaporating machine, without which no amount of mere cold will make snow. Apart from this, as Prof. Newcomb complains, Croll's language is wanting in quantitative precision, and he uses the terms " great," " very great," " small," " comparatively small," without any statement of the units of comparison relatively to which these expressions are employed. It is by the application of precise and definite numerical statements and calculations to the problem that Culverwell has shown the incompetence of Croll's postulate. The latter says, " with the very limited fall in temperature coincident with the greatest extension of eccentricity in the earth's orbit there would be no increase of snow worth speaking of, nothing that would not be melted by a couple of soft winter days, and Dr. Croll's coach won't move at all unless he is supplied with very severe winters and great additional snowfalls during that season in the time of excessive eccentricity ". This is in itself absolutely conclusive against Croll's position as it stands, and I do not see how that theory is to be modified to meet the crucial difficulty.

Let us proceed, however. Granting he secured his snow, how is that snow to be maintained? Inasmuch as Croll did not postulate any change in the distribution of land and water, but treated the problem as one dominated entirely by the present meteorological factors as modified and affected by an enhanced eccentricity in the earth's orbit, he had to face another initial difficulty in regard to the conservation of any enhanced snowfall. This has been pressed against him by his opponents, among whom none have written more ably than Prof. Newcomb and the Rev. E. Hill. The latter says : " We have no reason to suppose that at present, in the northern hemisphere, more snow or ice is anywhere formed in winter than is melted in summer. With greater eccentricity less heat than now would be received in winter, but exactly as much more in summer. More snow would therefore be formed in the one half of the year, but exactly as much more would be melted in the other half. The cooler winter and the warmer summer would exactly neutralise

each other's effects, and on the average of years no accumulation could begin " (*Geol. Mag.*, 1880, No. 12).

Croll does not dispute this. He frankly accepts it and himself says, " *Whatever the eccentricity of the earth's orbit may be, the heat received from the sun during summer is more than sufficient to melt the snow of winter* ". Again he says : " There is not a place on the face of the globe where the amount of heat received from the sun is not far more than sufficient to melt all the snow which falls upon it ". In view of this cònfession, and in view also of the fact that in order to set his meteorological machinery going at all, he had to find some method of accumulating the winter's cold instead of its being annually dissipated by the succeeding summer's warmth, he had to turn the flank of this and other similar arguments, and he did so in a very extraordinary way.

He did not meet the difficulty in the only way it can logically be met, namely, by showing that of the total heat which reaches the earth from the sun only such a small proportion is absorbed by it that it would be insufficient to counteract the tendency to snow-accumulation caused, as he argues, by excessive eccentricity with winter in aphelion. Nor does he give any credit to the two great snow eaters upon which he relies in other parts of his argument, namely, warm ocean and aerial currents, for making up very largely that portion of solar heat which is lost by radiation, and which would have entirely destroyed his case ; but he appeals for an answer to an entirely irrelevant phenomenon.

He urges that although enough sun-heat reaches the northern regions in summer to melt far more snow than accumulates there in winter, the amount of snow melted cannot be proportional in any way to the amount of sun-heat received or there would be no such thing as perpetual snow, and he goes on to quote the cases of the summits of the Himalayas and of the Andes and the plateau of Greenland, and he might have added the Alps and Dovrefeld, as cases where the snow never melts away entirely, because, as he very justly says, the greater part of the heat received there is reflected back into space and does not go to melt the snow or ice at all. This is true in itself, but surely it is an entire fallacy when applied by way of analogy to describe what

occurred during any glacial period. If Greenland remains
swathed with snow all the year round, the low islands *to
the north* of Greenland are stripped of all their snow every
summer. If the Himalayas are snow-clad, the vast tundras
of Siberia to the north of them are stripped of their snow in
the first fortnight of summer. The fact is, that to import
the conditions of high mountains and plateaux into the
question is to utterly delude the reader. The snow remains
on high mountains and high plateaux because the air there
is very dry and very transparent to heat, and thus the sun's
rays are radiated very largely into space ; but the condition
of things in the postulated glacial times was the very reverse
of this. The glacial period, according to Dr. Croll himself,
was only possible because the air was so full of vapour in
high latitudes where there was an excellent condenser, and
the presence of this vapour in the air prevented the radiation
of the heat into space in the way which takes place on high
and dry mountains. That is so obvious that it is extra-
ordinary how Croll could have pressed the argument and
illustration. Yet he seems to think he has entirely discom-
fited his enemies by it. As a matter of fact he has not
touched their case even. When we say that under any
conditions of eccentricity the sun-heat received by the earth
in summer must melt and dissipate the snow which has
fallen in winter, we do not also postulate that the earth as a
whole has the surroundings and conditions of high mountain-
tops. We are thinking of the great mass of the earth with
an atmospheric pressure of about thirty inches and with a
blanketing atmosphere largely charged with vapour. We are
thinking, in fact, of things as they are on the earth, and not
as they might be in the empyrean regions where the cirrus
clouds are formed. The fact is that Dr. Croll nowhere gets
over this initial difficulty, and it remains an insuperable answer
to all his special pleading.

The utter breakdown of Croll's successive postulates does
not stop here. Having secured, as he fancied, an abnormal
accumulation of snow in some winter, he continues : " By the
end of summer the snow would all disappear except on high
mountain-summits, such as those of Scotland, Wales and
Scandinavia. Before the end of autumn, however, it would

again begin to fall. Next year would bring a repetition of the same process, with the difference, however, that the snow-line would descend to a lower level than in the previous year. Year by year the snow-line would continue to descend, till all the high grounds became covered with permanent snow." These conclusions I fail altogether to follow. The colder winter would be accompanied by *a hotter summer.* The more condensed summer-heat would (except when the air was sufficiently diathermatous) be a more potent instrument of melting, and the fact of the air being full of humidity, and not therefore very diathermatous, is a necessity of the case. How under these conditions a greater quantity of snow would remain on the mountains than remains now I do not see, and if it did so in some unusual winter, why should the snow fields increase and the glaciers increase *ad libitum* any more than they do now after a hard winter? If an initial reservoir of ice and snow is enough to make them so increase there ought to be no retrogressing glaciers now. The case of Mars is also very instructive in this behalf. There, enormous ice sheets are formed and dissipated every year, but no accumulated ice age is the result. It seems to me that Croll has on this question met the objections of Hill and Newcomb, which are really the same as those of Arago long ago, by delusive fallacies, but in addition he has committed the supreme fallacy of altogether ignoring as a most potent factor in the problem what he elsewhere makes so much of, namely, the effect of convection of heat to high latitudes by air and water currents, and has treated the whole problem as a mere local battle of sun *versus* snow and ice.

So much for the abstract position. I will now quote at considerable length the more concrete criticism of a very accomplished meteorologist who has pulverised Croll's theory from the meteorological side, namely, Woeikof. Woeikof first points out a singularly misleading postulate of Croll, which is embodied in his statement that " the ocean must stand at a higher mean temperature than the land ". As his critic says, inasmuch as mean and not surface temperature is mentioned, there can be no mistake about his meaning, and that meaning is quite inconsistent with the facts. " Not only have the oceans, which receive cold currents from polar

seas, a much lower mean temperature (by mean temperature meaning that of the whole column of water) than the land, but even seas receiving no such cold currents, as the Mediterranean and Red Seas, have a mean temperature considerably lower than the land." Woeikof next criticises Croll's statement about the difficulty in the sea of "getting quit of its heat as rapidly as the land," and points out that he seems to entirely overlook the mobility of the particles of water by which convection currents are set up as soon as the surface temperature sinks below that of the stratum immediately under it, bringing the latter to the surface, and thus always maintaining a higher temperature at the surface. He seems to consider only the loss of heat sustained by the ocean by radiation, and he points out that wherever, like the ocean, the mass of water has a higher temperature than that of the maximum density, the convection currents are conducive to a loss of heat by the whole mass, as the colder water sinks to the bottom, where it is out of reach of the radiant heat of the sun and receives heat only by the slow process of conduction.

Turning to the air, Woeikof maintains, contrary to Croll, that the loss of heat by the air by radiation is very slight. The loss of the earth's heat by radiation is not really in any effective sense from the air, but from the surface of the land and water. He further urges, against what is another conclusion of Croll, that the air is in a state of stable and not unstable equilibrium. If it rises it gets cooler by an amount correlevant with the amount of expansion, and if it sinks it gets warmer by the correlevant amount of its compression. This creates an equilibrium which has been tested by many experiments. If it were not so there would be continual convection currents which would bring a temperature of $32°$ to the sea level. An unstable equilibrium does exist, except in thunderstorms, hailstorms, etc., in the lower strata up to a few thousand feet, and this only in the daytime, when the ground is much heated by the sun. It disappears about sunset, and does not occur in winter in high or even in higher middle latitudes (say from $50°$ northwards). Woeikof urges that a temperature of $-50°$ F. would be found at such a height above sea level that if the air were forced down it would arrive warmer and not colder than the air at the sea level.

Turning to fogs, Woeikof contests Croll's notion that fogs in high latitudes are due directly to the melting of the snow. He says that the melting of the snow by the sun has not by itself the power to cause fogs, and that on extensive continental regions in Europe, Asia and America the snow is melted from March to June, yet fogs are exceedingly rare and generally occur at night, and are frequent in autumn during anti-cyclones. "The cause of these fogs," he says, "is the same as that of the London fogs, that is, the temperature of the river or lake water is much higher than that of the air, and thus the vapour is soon condensed. Fogs do not commonly occur in summer on mountain glaciers when the melting is very great, but they are common on the sea near melting ice, because then we have two masses of air of unequal temperature, both nearly saturated, and their mixture must produce saturation, that is, fogs. As interceptors fogs do more work in preventing radiation from the earth than in screening the sun's rays. Thus, says Woeikof, fogs are not necessary followers of the melting of snow and ice *per se*, some other conditions are necessary for them.

Again, Croll, in speaking of the south winds which blow over Siberia, treats them as hot winds. So they are in summer, but in winter, when they chiefly prevail, they are icy cold winds, having come from the icy plateau of Tibet. Croll's notion was that if south winds prevailed more than they do now in winter in Siberia this would greatly increase the snowfall. This is not so : the snowfall of winter there must be exceedingly light, since these south winds are descending winds and have become relatively dry. Woeikof quotes actual observations for these facts. He next tests Croll's theory by certain examples. Thus, admitting that in times of great eccentricity the mid-Atlantic midwinter temperature was 6° higher than in Great Britain, it would follow from Croll's argument that in the time of the greatest possible eccentricity 850,000 years ago, with winter in aphelion, its mean temperature would be – ·03° F., and 210,000 years ago it would be 7·3° F.,[1] that is, temperatures which would be possible only if the ocean were covered with solid ice, which is an impossibility with anything like the present geographical conditions, and

[1] Woeikof has made a mistake in his figures which I have corrected.

Croll admits repeatedly that they have not changed since the glacial period.

Again using the calculations of Ferrel, the mean January temperature in 50° and 60° N. lat. respectively is 21·3° and 1·7°. Now the amount of heat received at the winter solstice in 60° N. lat. is but 0·35 of that received on 50° N. lat. Thus, says Woeikof, if the temperature of the former were less in proportion to the quantity of solar heat received, it would be – 147° F., which is a vast discrepancy. Croll attributes the small decrease of temperature with latitude to the influence of currents of warm water from the tropics, but on latitude 60° N. there is considerably more land than on 50° N., and air over the land in latitude 60° N. is colder than over the sea. If, still following Ferrel's tables, we take the case of the very continental climate of eastern Siberia, the mean January temperature of 50° and 60° N. lat. respectively is 0° F. and – 30° F.; according to Croll's hypothesis it ought to be – 155° F. on 60° N. Lat.

Woeikof then takes the most favourable possible conditions for Croll's view and tests the case by them. He takes the highest January temperature in 20° E. in 50° N. lat., and the coldest January temperature in 120° and 130° E. and 60° N. lat., the former being 44° F. and the latter – 30°. If tested, as Croll tests the problem, by the quantity of heat received, the mean temperature in January in 60° N. lat. should be – 140° and not – 30°. These tremendous discrepancies in the case of strictly continental climates show how much Dr. Croll's calculations for Great Britain must be wrong. In so oceanic a climate an equal difference in the amount of sun-heat would certainly cause a smaller fall of temperature.

After quoting some other cases of discrepancy Woeikof says: " All this I think conclusive enough, and proves that *Dr. Croll's system of estimating temperatures breaks down when tested seriously.* Small errors would be quite natural in a question of that kind, but I have shown that *the errors are enormous,* amounting to 100° F., and more; that is, they are *greater than the difference of annual temperature between the Equator and the North Pole.* There is certainly a mistake somewhere; or rather the whole method is a failure." Woeikof very properly calls attention to the enormous uncertainties attending the very elements of the problem, among them perhaps

the most important being the different diathermancy of the air under different conditions according to the quantities of carbonic acid or aqueous vapour it contains, or the quantity of suspended liquid and solid particles in it (clouds, dust, smoke, etc.). The uncertainty here involved is quite as great and as important as that about the true temperature of space. The calculation of Dr. Croll gives figures for the minimum temperature in the latitude of 60°, and gives lower figures than the extreme minimum anywhere observed by reliable thermometers, which is about − 81·4° F. Neither in the coldest parts of eastern Siberia nor in the highest latitudes of Greenland and Grinnell land have lower temperatures been noticed, and yet in Flacberg Beach the sun is absent from the horizon more than four months, and the lower we make the temperature of space the more conspicuous is the tenacity with which the surface of the earth and the lower stratum of air retain a relatively high temperature. So when Dr. Croll says that the stoppage of all currents would make the mean temperature of the equator 135° F., he postulates a heat in excess even of a single month anywhere. The absolute maximum known by exact observations does not exceed 121° F.

Woeikof altogether questions the cooling effects of winds from the middle latitudes upon the equatorial region, and shows that both north and south of the equator the winds which reach it are warm winds. In latitude 10° N. of the equator the mean temperature is actually 81° F., and 10° S. it is 78·7° F., while the equatorial mean is only 80°. The cool winds from middle latitudes have been warmed long before they reach the equatorial zone.

He similarly protests against Croll's statement that the mean temperature of the equator would be enhanced 55° above what it is now were it not for the heat-abstracting action of ocean currents, and calls it mere speculating *à perte de vue* and as ignoring some of the best known facts of climatology. He shows that in the upper Amazons there is no such heat-abstraction and that aerial currents can have no cooling influence even of a degree Fahrenheit, and yet the mean temperature reduced to sea level is not anything like 135° F., but below 80°·

Again Dr. Croll has overlooked in a great measure the great difference of continent and ocean in the matter of tem-

peratures. " The caloric capacity of water is so great and the mobility of its particles so effectual in resisting a lowering of the surface temperature by the convection currents it causes, that it seems very doubtful if during a great eccentricity with winter in aphelion the surface temperature of the ocean would be lower in winter than now, and if the snowfall would be greater. In regard to the interior of continents, Woeikof is disposed to think the effect might be appreciable. "But," he adds, " what has this to do with glaciation? Even now the temperatures in the interior of large continents are low enough in midwinter to allow of the snow remaining on the ground for some weeks, not only under 45° N. but even under 40° N. And yet we have no glaciers on the North American continent which reaches to 71° N., and on the Asiatic which reaches to 78° N., except in high mountain regions, because the snowfall is so small that it is melted in summer. Even the mountains of north-east Siberia have no glaciers. . . . The winter snowfall in Siberia is very small, and a further lowering of the winter temperature there would diminish it further and leave less snow for the summer sun to melt instead of more. In Transbaikalia, with a mean winter temperature of – 13° F. and below, there is generally too little snow for sleighing, and where there is more snow, as near the Crimea, it is at the beginning of the cold season in October and November, when the east winds bring warmer and moister air from the seas not yet then frozen. On the contrary, the high temperature of the great plateau in summer causes rain and not snow to fall on heights of 15,000 feet. Thus the heavy rains of summer are not favourable to snow accumulation, and rather assist in melting the small quantity of snow on the ground, and this would be intensified with high eccentricity and winter in aphelion, when the summer temperature would be higher and the snow would melt at higher levels. At present in north Tibet permanent snow is found at from a height of 20,000 feet " (*Amer. Journ. of Science*, 1886, p. 161, etc.).

Again, he says " a greater cold in winter would not be conducive to an accumulation of snow, while a more intense heat in midsummer would probably melt the snow at heights where the present temperature does not rise much above 32° F. In the monsoon regions a colder winter in the interior, with the

accompanying higher pressure of the air, would intensify the cold and dry monsoon winds, and thus bring about conditions even less favourable to an accumulation of snow. Greater heat in summer in the interior of Asia would intensify also the moist summer monsoon, and thus give a greater amount of precipitation. But owing to the small amount of snow falling in winter and its rapid melting, the temperature would rise over 32° F. even at heights considerably greater than now, and the precipitation due to the moist winds would be rain. Thus, in the interior and eastern part of a continent like Asia, winter in aphelion during a high eccentricity would be less favourable than even the present conditions to permanent snow and glaciers.

" As to the western parts of continents and to islands, they are more fully under the influence of the seas. As there is no reason to suppose that the surface temperature of the sea would be lower during winter in aphelion and high eccentricity, it follows that there will not be more snow than now in countries where rain is the rule even in winter, all other things being equal. As there is also nothing in these astronomical changes to intensify the moist, (principally westerly) winds in winter, there will also not be a greater quantity of snow falling at that season in regions having a regular covering of snow in winter. The greater heat and rarefaction of the air in the interior of continents in summer will cause the air of the oceans to flow thither with greater force, and such a movement of the air is favourable to more abundant summer rains than are experienced now, and thus to a melting of the snow in mountainous countries.

" Thus it would seem that winter in aphelion during high eccentricity would have rather the opposite effect to that which is generally attributed to it, but it seems to me that the effect would be in any case very slight, and not by far to be compared to that of the distribution of land and sea, mountains and lowlands ; in other words, to that of geographical conditions " (*Nature*, vol. xxv., pp. 425, 426).

Coming nearer home in his criticism, Woeikof says : " If a high eccentricity and winter in aphelion can have a material influence on climate, it would give to Western Europe colder winters, with a greater proportion of dry east winds, and

warmer summers, both conditions unfavourable to glaciation. The Ural Mountains, as well as those of eastern Norway, have prevailing west winds and a much colder winter, but on account of the small snowfall no permanent snow and no glaciers, while the west side of the Scandinavian peninsula has enormous glaciers."

Again, Woeikof argues that, inasmuch as he has shown that greater eccentricity with winter in aphelion would not cause any considerable lowering of temperature on the ocean in winter, it would not, as Croll urges, bring any change in the velocity of the Trade winds and their more southerly extension when winter in aphelion exists in the northern hemisphere, and the reverse during winter in aphelion in the southern hemisphere. Woeikof concludes a masterly analysis of a difficult problem with the pregnant words : "An English geologist of note (*i.e.*, Searles Wood, junr.) has called Dr. Croll's hypotheses brilliant and fascinating. So they certainly are. The originality of the conception, the fertility of resource of the author, his indomitable will are sympathetic in the highest degree. It is with a melancholy feeling that I confess that, interesting and important as are some parts of the system of Dr. Croll, the main points of it are opposed .to the most certain teachings of meteorology and cannot be accepted. . . . The wind theory of the upper oceanic currents, the notion of the great climatological effects of these currents to the exaggerated extent given them by Dr. Croll, and some of his considerations on the conservative effects of snow and ice—*the main points on which rests, so to say, the whole fabric in its explanation of glaciation and geological climates generally—the influence of winter in aphelion and perihelion during high eccentricity, and the calculation of temperature in proportion to the sun-heat received, are unfortunately fallacies.* Geologists will have to look for other causes to explain the more or less frequent glacial and interglacial periods which their studies lead them to expect " (*Amer. Jour. of Science*, 1886, pp. 161-178).

Let us now turn to the second great factor in Croll's theory, namely, his argument in favour of a transference of glacial and mild conditions from one hemisphere to the other in turn. Upon this I wrote at great length in my *Glacial*

Nightmare, and, as I believe, the analysis of Croll's position I there offered is unanswerable. It has not, at all events, been answered anywhere.

The cardinal position taken up by Croll is that the great ocean currents being in a large measure the result of the wind currents which impel them along, if we can divert the wind currents or alter their intensity and strength we shall at the same time alter the direction or intensity of the ocean currents. This was a perfectly sound postulate. Croll then went on to argue that the equatorial calms are the result of the meeting and mutual neutralisation of the Trade winds blowing from the north and the south of the equator respectively. This view I ventured to show is entirely erroneous. The region of calms is the region where the earth's mean heat is the highest, where, therefore, the terrestrial furnace is doing its most effective work, and where the heated air, instead of blowing hither or thither, rises into the upper regions of the atmosphere as in a natural flue on a great scale, and then passes on to the north and to the south respectively. The position of the belt of calms in regard to the earth's equator of form is, according to Croll, dominated by the respective potencies of the north and the south Trades, and its mean focus is now considerably to the north of the equator, because the south Trades are more potent than the north Trades. This seems to me to be an entire fallacy. The situation of the calms is dominated, not by any supposed potency of the Trades, which instead of neutralising each other do very good dynamical work in driving the great equatorial current round the equator from east to west, but by the position of the earth's equator of heat, which does not coincide with its equator of form, but happens to be elsewhere, and which fixes the region of calms in a belt of which it is itself the focus, where the natural furnace must necessarily be. This completely breaks down and dissipates another great factor in Croll's position, since it detaches altogether the position of the calms from any potency or otherwise of the Trades.

Let us grant, however, Croll's postulate. He then proceeds to argue that, with extreme eccentricity and winter in aphelion in our hemisphere, the potency of the present southern Trades

would be transferred to the northern ones and the belt of calms would be driven to the other side of the equator, and with this would also be driven thither the flow of large quantities of warm water inducing milder conditions than those prevailing previously in the other hemisphere. This involves a second fallacy. The present position of the equator of heat is generally explained as the result of the present preponderance of land in the tropical region north of the equator compared with the conditions south of the equator. This view is supported by many inquirers of weight. If it be just, it is clear that we cannot move it to the south unless we also materially alter the relative position of sea and land in the equatorial region, so as to make land surfaces prevail over water surfaces in the area south of the equator. But Croll has argued strongly against any material geographical changes of this kind in this area since the so-called glacial period, and I suppose every geologist would support him.

I ought here to correct a mistake in my former volumes. In speaking of the position of the equator of heat in our summer, I put it at 10° N. of the equator, which is quite right if we limit ourselves to a considerable part of the equatorial region, but it is not true of the whole equatorial belt. The region of calms moves north and south with the sun. In the Atlantic and in the eastern Pacific the region of calms lies at all seasons north of the equator, and travels from about 11° N. in August to 1° N. in February, its breadth varying from 3° to 8°; but, as Buchan points out, "in the western division of the Pacific Ocean the region of calms travels to the south of the equator in the summer months of the southern hemisphere" (*Encyclopædia Britannica*, xvi., pp. 143, 144). This is apparently also the case in the Indian Ocean. This is due, doubtless, to the effect of the large amount of land south of the equator in the Australian continent, the Eastern Archipelago and India. This position of the belt of calms and its movement with the sun are only consistent, it seems to me, with its being dominated by the position of the equator of heat quite irrespective of the winds. It is the belt of calms which is grouped about the equator of heat which prescribes the position of the Trade winds, and not *vice versâ*.

Pilar, who claims that the real climatic equator is the line where the mean summer and winter temperatures approximate the most, has made a series of calculations which seem to go to show that even in the period of greatest eccentricity the equator of heat would still remain north of the equator, since he shows that the summer and winter temperature would then approximate most closely about lat. 20° N. instead of 5° N., as it does now. The following table condenses his results.

Northern Latitude	I. ECCENTRICITY—0·07775.			II. ECCENTRICITY—0·01672.		Northern Latitude
	Perihelion Winter in the Northern Hemisphere T = 1·3656.	Difference between Summer and Winter.	Aphelion Summer in the Northern Hemisphere t=1.	Difference between Summer and Winter.	Perihelion Winter in the Northern Hemisphere T=1·0688.	
65°	0·03693	− 0·71144<	0·74787	>0·71927 −	0·02891	65°
60°	0·15577	− 0·64756<	0·80833	>0·68142 −	0·12191	60°
55°	0·27342	− 0·57876<	0·85218	>0·68819 −	0·21399	55°
50°	0·38899	− 0·50555<	0·89454	>0·59010 −	0·30444	50°
45°	0·50160	− 0·42849<	0·93009	>0·53851 −	0·39258	45°
40°	0·61039	− 0·34818<	0·95857	>0·48084 −	0·47773	40°
35°	0·71454	− 0·26521<	0·97975	>0·42051 −	0·55924	35°
30°	0·81324	− 0·18023<	0·99347	>0·35698 −	0·63649	30°
25°	0·90578	− 0·08385<	0·99963	>0·29073 −	0·70890	25°
20°	0·99094	=0·00724=	0·99818	>0·22226 −	0·77592	20°
15°	1·02110	>0·03196 −	0·98914	>0·15231 −	0·83683	15°
10°	1·13940	>0·15683 −	0·97257	>0·08080 −	0·89177	10°
5°	1·20006	>0·25260 −	0·94860	=0·00888=	0·93972	5°
0°	1·25280	>0·33539 −	0·91741	− 0·03859<	0·95600	0°
5°	1·29540	>0·41617 −	0·87923	− 0·13463<	1·01386	5°
10°	1·32810	>0·49374 −	0·83436	− 0·20512<	1·03938	10°
15°	1·35070	>0·56773 −	0·78297	− 0·27422<	1·05719	15°
20°	1·36810	>0·63713 −	0·72598	− 0·34088<	1·06686	20°
25°	1·36510	>0·70193 −	0·66317	− 0·40523<	1·06840	25°
30°	1·35660	>0·76108 −	0·59552	− 0·46630<	1·06182	30°
35°	1·33790	>0·81466 −	0·52324	− 0·52391<	1·04715	35°
40°	1·30900	>0·86303 −	0·44697	− 0·57755<	1·02452	40°
45°	1·27010	>0·90279 −	0·36731	− 0·62677<	0·99408	45°
50°	1·22150	>0·93665 −	0·28485	− 0·67123<	0·95608	50°
55°	1·16370	>0·96348 −	0·20022	− 0·71059<	0·91081	55°
60°	1·09700	>0·98292 −	0·11407	− 0·74353<	0·85860	60°
65°	1·02190	>0·99495 −	0·02705	− 0·77281<	0·79986	65°
Southern Latitude	Summer in the Southern Hemisphere.	Difference.	Winter in the Southern Hemisphere.	Difference.	Summer in the Southern Hemisphere.	Southern Latitude

In the preceding table the first and last columns contain a list of latitudes from 65° N. to 65° S. at intervals of 5°. The central column represents the mean summer temperature of each zone with summer in aphelion, which is taken as constant in all conditions of eccentricity; the second and sixth columns represent the mean winter temperature with winter in perihelion now and in the period of greatest eccentricity respectively. Columns three and five explain themselves. T means the mean winter temperature if we take 1 as the mean summer temperature, which is represented by t.

From whichever way, therefore, we approach the question, it seems quite clear that the notion of Croll that the belt of calms and the equator of heat could be driven to and fro, north and south, by the alternately greater potency of the north and south Trade winds is quite transcendental and untenable. It is also repudiated by Sir R. Ball, the other protagonist of extreme glacial views based on similar premises, who says of it: "I have been unable to perceive the validity of the arguments by which Dr. Croll strives to show that a great ocean current like the Gulf Stream must be actually diverted from one hemisphere to the other as a consequence of the transference of the glaciation from one hemisphere to the opposite. I cannot think this is likely" (*A Cause of an Ice Age*, p. 133).

Mr. J. J. Murphy, while adopting the greater part of the reasoning and conclusions of Croll, reversed his main contention. Instead of arguing that great glaciation would result in times of great eccentricity on the hemisphere whose winter was in aphelion, he argued that this would take place when the summer was in aphelion, that is, when the summer was long and cool, and the winter short and warm. He says: "The extent of glaciation depends in no degree on mean temperature, but exclusively on summer temperature. The 'snow-line' is the line of summer snow, and theory and observation agree in showing that the extent of glaciation depends chiefly on the height of the snow-line so defined. There is a region in eastern Siberia where the ground, at the depth of a few feet, is frozen all the year round, showing that the mean temperature of the year is below frost, and

yet over that frozen soil cattle graze, crops of rye are harvested and pine forests flourish. It is obvious that if from any cause the extremes of that climate were to disappear, while its mean temperature were to remain unchanged, so that there was a temperature, below freezing for every month of the year, all the precipitation would be of snow, which would remain unmelted, and the land would be covered with continual ice like Greenland."

Whatever force this kind of argument may have as against Croll, whose logic at times is not unlike it, it has no substantial soundness, and can be tested in a very few sentences. Instead of the temperate regions being snow-covered like Greenland, as Mr. Murphy urges, there would probably be hardly any snow at all, for there would be no evaporation and the air would be dry. We must have damp air to produce snow, and damp air is only steam, and requires heat to manufacture it and not cold.

But all these speculations fail most when tested quantitatively. As we all know, the snows of Siberia are now melted clean away in a very few days of early summer; and if Mr. Murphy would have tried to realise what a portentous change in the summer would be required in order that the snow there should survive, not a few days of summer, but the whole summer; and what a change it would require to convert the hot summer south winds of Siberia into cold winds—what enormous changes of geography as well as climate—he would hardly have ventured upon his theory. Like others he ignores the heat brought into the north by convection currents, and thinks only of the direct sun-heat; and he forgets also the ocean areas, while he converges attention on the continental ones. But the fact is, the whole theory falls to pieces like a pack of cards because it is mainly based on Croll's conclusions and arguments, which we have shown to be untenable.

Manipulate and juggle as we will with the figures and the oftentimes very intangible facts of meteorology, the one supreme difficulty remains that, under any possible conditions of combined astronomy and meteorology working in their normal methods, we cannot secure such an accumulation of snow or ice in winter as will maintain itself against the

masterful potency of the succeeding summer. Murphy's is in reality only a modification of Croll's theory based upon his arguments, and differing only in one of its factors, in which Croll has the best of the argument (see *Glacial Nightmare*, 416).

If we want an object-lesson of the best kind to test the combined astronomical and meteorological theories of Croll and Murphy by, we cannot well have one more to our purpose than the planet Mars. The astronomical conditions in Mars are much more favourable as tested by Croll's conditions for the creation of a glacial epoch than they ever were or could be on the earth. This, both in regard to the obliquity of its axis, which is now in excess of the greatest obliquity which could prevail on the earth, namely, 28·51°, and also in regard to the eccentricity. This also is far in excess of the greatest eccentricity postulated by Croll. It is approximately 0·09326, while that of the earth at this moment is 0·01617, and when at its greatest possible maximum was only 0·0777.

The perihelion and aphelion distances also greatly vary. At the southern summer solstice the earth is about 3,000,000 miles nearer to the sun than in midwinter, but Mars at the same season is 28,000,000 miles nearer than at the southern winter solstice. Hence the lengthening of the latter season. On the earth the southern summer is eight days shorter than the winter, on Mars the difference is seventy-four days, the planet spending 306 days in the perihelial and 380 days in the aphelial division of its orbit. On the other hand, as on the earth at this moment, " the winter solstice in Mars of the northern hemisphere coincides with perihelion and its summer solstice with aphelion, and the winter solstice of the southern hemisphere coincides with aphelion, the summer solstice with perihelion ". If eccentricity were the main cause of glaciation, Mars ought to be most favourably situated for it. If eccentricity were in effect a factor of any moment, on a planet with nearly the same inclination as the earth, but an orbit six times as eccentric, there ought to be ample evidence of it in its antarctic snows. These being the astronomical conditions, let us now turn to the geographical ones.

During the diminution of the Martian snow caps a dark band of unequal breadth, but averaging in June, 1894, about 200

miles, borders each of them. This dark band, according to Mr. Lowell, of the Flagstaff Observatory in Arizona, is water beyond doubt, for it is of the colour of water, it faithfully follows the melting of the snow, and it subsequently vanishes. These independent facts are mutually confirmatory of this conclusion.

Having discriminated land and water surfaces in Mars, we may next consider their distribution. As Mr. Carpenter says " The distribution of land and sea on Mars is far more equable than on the earth, and there is a remarkable symmetry in this respect between its two hemispheres. At neither pole is there apparently an ocean of great extent, and, as is well known, land prevails over sea throughout, the two being in an estimated proportion of three or four to one. Thus, as far as we can judge, geographical causes of difference between the poles seem to be next to eliminated in Mars, and we are left to observe the results of the purely astronomical influences." (*Geol. Mag.*, 1877, p. 98). Let us now turn to the meteorological conditions.

The air of Mars must be very thin, since delicate tests have many times failed to detect it; but the peculiar hazy appearance that veils the structures near the edge of the disc justifies Mr. Lowell's conclusion that a thin atmosphere exists, and as there are snow and water, there must also be watery vapour, of tension varying with the temperature. Assuming other conditions as on the earth, says Claypole, this tension would at 32° F. equal 0·2 of the mercurial thermometer, and at 80° F. it would equal an inch. One curious effect must be the rapid transfer of the vapour from the sunny to the shady side of the planet. The rapid evaporation that must accompany the high temperature of the Martian day must produce an aqueous atmosphere of considerable tension, which must immediately flow off more or less completely to the opposite hemisphere, and there condense, probably at once to the solid form. Mr. Claypole says: " The relative amount of heat and light received by the earth and Mars is about 9 to 4. Notwithstanding this, and probably due to the tenuity of its atmosphere and the consequent absence of fog and cloud, the scarcity of water may keep its poles above the freezing point in summer and cause its polar ice to disappear. The

aqueous vapour must retain near the planet's surface the reflected solar heat, and thus cause the lower layers of the atmosphere to be considerably raised by day, and their cooling at night by radiation to be equally reduced. Hence the summer climate may be anything but intolerable, and the continual flow of vapour and its condensation may also greatly mitigate the severity of the long winter of twelve months. If, as Schiaparelli says, the northern snow cap does not begin to increase until a month after the occurrence of the northern vernal equinox, it follows that the shady side of the planet cannot be intensely cold, or that the quantity of water thus transferred is not very great. Both conclusions are probably true. The density of the Martian atmosphere, which is heavily charged with vapour, is considered by Mr. Lowell to be less by a half than that of the air at the summit of the Himalayas. This would equal a mercurial column (on the earth) of about three inches. Under such conditions movement would be easy and the transfer of gas or vapour from place to place exceedingly rapid " (*Am. Geol.*, xvi., p. 91, etc.).

As is well known to every tyro in astronomy, Mars has a white cap at each pole; these have been generally considered to be snow caps. As Prof. Claypole says: " It is difficult to avoid the conclusion that the white masses on the poles of Mars as they emerge from their long winter are really snow caps. Their constant occurrence, their regular seasonal diminution during the past two hundred years as the poles alternately come out into the strong sunlight, and the total disappearance of one of them for the first time since observations began during the last Martian summer of 1894, form an argument so strong as to be almost demonstrative in support of this long entertained opinion " (*Am. Geol.*, xvi., p. 91).

The behaviour of these snow caps is also singular. During their maximum in winter they are of nearly equal magnitude, which they ought not to be according to Croll's view. In summer, however, they are very different: while the northern cap varies slowly and little, the southern one varies rapidly and largely. In the year 1830 the southern snow cap was observed during the midsummer of Mars to diminish to half

its former diameter in a fortnight. Thus on 23rd June it was 11° 30″ in diameter and on 6th July it had diminished to 5° 46″, after which it rapidly increased again. According to Schiaparelli, in 1877 the south pole of Mars was entirely free from snow. It was free again in 1894. Meanwhile the cap on the northern hemisphere was three or four times larger, a fact which Mr. Carpenter and Mr. Belt held, and surely with reason, to be opposed to Croll's view that the hemisphere which has its winter in aphelion (as the southern now has in the earth and Mars) was alone glaciated during periods of high eccentricity, while *pro tanto* it favours the view urged by Murphy.

Mr. Claypole argues in the same way. " Thus," he says, " in consequence of the great inclination of the axis of Mars, its north pole is in the sunlight in the long aphelion passage of twelve months. These are precisely the conditions deemed so favourable by Croll for one of his warm interglacial climates " (*Climate and Time*, p. 237). Instead of this, however, we find the north pole of Mars capped with its snow and ice as regularly and almost as extensively as its south pole, and this wintry accumulation lasts as long into the northern summer as does its southern counterpart into the warm season of the southern hemisphere.

Instead, therefore, of an icy perpetual winter overwhelming one hemisphere of Mars while the other is enjoying a genial climate, as should be the case if Croll's contention was right, we find a corresponding snow cap in each hemisphere.

Mars affords no evidence, in fact, in support of the eccentricity theory of glacial cold, though his conditions are at present such as to favour a state of extreme glaciation in his southern hemisphere.

Again, we do not find a generally severer climate in Mars than on the earth, as we ought to do if the eccentricity theory were true. His snow caps extend no farther than do those of the earth. They do not indeed extend so far. In a severe American winter the snow field is often continuous from the pole to the middle States, or to about the parallel of 35° N.; not infrequently the whole northern part of the continent is sheeted in white. Nothing of the kind has ever been seen in Mars, yet a snow sheet of two or three inches

would be just as conspicuous as one of greater depth. It would seem, in fact, that the climate of Mars, though farther from the sun and with greater eccentricity, is milder than that of the earth. (Claypole, "Glacial Notes on the Planet Mars," *Am. Geol.*, xvi., p. 91, etc.)

No true glacial conditions, in fact, exist in Mars. His polar snow caps form regularly every twenty-two months, and have done so for two centuries past, but they never extend beyond the frigid zones of the planet, and with the summer sun they waste and dwindle, and, as in 1894, even disappear. Not so on the earth. No marked reduction of the antarctic ice cap occurs in the antarctic summer. The corresponding season in Mars is ten months instead of six, but the corresponding winter lasts a whole year. Water may, however, be so scarce in Mars, as Wallace argues, that Mars has no vast and permanent ice sheets over her temperate zone, as the earth is supposed to have had by the extreme glacialists. This he reasonably claims to be shown by the *rapid* disappearance of the white patches over a belt three degrees wide in a fortnight (equal to a width of about 100 miles of our measure), and in the northern hemisphere of eight degrees wide (about 250 miles) between the 4th May and the 12th July. He compares this phenomenon with the rapid disappearance of the snow in Siberia, proving that in Mars as in Siberia the snow covering is only a thin one. This is especially notable inasmuch as our sun is so much more powerful than that of Mars. The case of Mars is singularly in unison with the views of those who claim that any increased cold of a winter in aphelion must be undone by a fiercer sun in perihelion. It is probably because the Martian summer has a third more heat in its southern than in its northern hemisphere owing to its occurring in perihelion that it is able to undo so much more of the effect of the winter's cold there than in the other hemisphere. At all events its witness is clearly entirely against the arguments of Croll, according to whose views it ought to be enduring a severe glacial climate in its southern hemisphere, and a mild interglacial climate in its northern one.

While the case of Mars affords no support whatever to Croll's theory, it does coincide with *a priori* expectations of what ought to happen, namely : " that that pole which

endures the extremely hot summer and the extremely cold winter should present the extreme of fluctuation in its snow cap, and that that pole which endures the moderately hot summer and the moderately cold winter should present a kind of moderation or mean in the fluctuation of its snow cap. . . . They tell us in fact in unmistakable language that, other things being equal, our northern hemisphere and not our southern one ought now to be glaciated " (*Geol. Mag.*, 1877, p. 99).

I do not propose to carry the analysis of Croll's theory and its modification, to which I have devoted a great space already elsewhere, any further. It seems to me, wherever tested and however tested, to be vitiated by fundamental fallacies, and to be quite worthless ; and instead of having, as Mr. James Geikie says, undoubtedly thrown a flood of light upon our difficulties, it has only enveloped those who have trusted it in impenetrable fogs. If it is to be quoted in the future it is quite time that it should be refurbished and repaired, if such a process is possible ; and some of Croll's old friends at the Geological Survey who are still loyal to him would perhaps do well to erect a most fitting monument to his memory by going out like David and smiting the Philistines who have laid low their hero, their hero who was the real scientific father of the glacial nightmare in its most extravagant form.

The great mass of geologists are looking elsewhere. In America Croll's theory has no longer, so far as I know, any adherents, nor has it among the great mass of European geologists. It retains a hold, however, upon the Geological Survey of Great Britain, in whose bosom it was hatched, and where it is the fashion to ignore other than official opinion and criticism, and to go on repeating fallacies, decades after they have been entirely exploded and abandoned elsewhere. The greatest sinner of all in this respect is the person to whom most of us are under the deepest obligations, even when we differ from him most, namely, Dr. James Geikie. It is almost incredible that in the last edition of his great *Ice Age*, which is an imposing monument of labour and reading, not a word should be said about the criticisms of Culverwell and Woeikof, which have entirely eviscerated and

demolished Croll's theory, not to speak of the efforts of the writer of these pages in the same behalf.

In answer to Newcomb's unanswerable dilemma, Croll, who had vainly tried to fence with it, as we have seen, gives us no kind of genuine reply. In a note he adds, however, the plaintive and ominous statement : " Were this objection correct it would prove that there could have been no glacial epoch; for it is obvious that had not the sun's heat failed to melt the winter's snow, not during the course of a few days merely but during the entire summer, there could not possibly have been permanent ice ". Exactly so, and that is one of the reasons why one voice in the wilderness at all events continues to denounce the glacial theory of so many writers as a glacial nightmare.

Note I.—Since finishing the previous chapters I have met with the following note which I think it opportune to quote. It embodies the view of Professor Adams, the most famous of recent English astronomers. Writing in February, 1866, he said : " I do not myself believe in the change of eccentricity of the earth's orbit being a cause of climatic changes in the earth. The effect, if any, would depend only on the *square* of the eccentricity; and this always remains so very small, that I believe the effect on the earth's mean temperature would be almost insensible. Depend upon it, geologists who look in this direction for the cause of glacial epochs are entirely on the wrong tack " (*Geol. Mag.*, 1895, p. 142).

Note II.—The criticism of the views of Croll and Murphy in the previous chapter is supplementary to that in chapter x. of my *Glacial Nightmare*, pp. 377-416, and is meant to be read as completing the argument there rather than as stating it completely. I may add that a good test of the futility of Murphy's modification of Croll's theory may be found in Pilar's table (*ante*, p. 79), which is based on the same premises as his, but gives us precise numerical results.

CHAPTER III.

VARIOUS THEORIES OTHER THAN THOSE OF BALL AND CROLL WHICH HAVE BEEN PROPOSED AS EXPLANATIONS OF AN ICE AGE.

"Bis zur Stunde müssen wir eingestehen; dass wir die tiefere Ursache der Eiszeit noch nicht kennen, so vielerlei verschiedene Gründe uns denkbar erscheinen mögen. Die Lösung auch dieser Frage ist der Zukunft ueber bunden" (Heim, *Handbuch der Gletscher Kunde*, p. 560).

IN the two previous chapters I have tried to analyse, as far as I have been able, the conditions affecting climate due to the known changes in the relative position of the sun and earth, and of the meteorological agencies which they set in motion; and I claim (largely by the help of other inquirers) to have shown not only that the views of Ball and Croll are untenable and unsound, but that, so far as we can see, no astronomical changes of the kind mentioned, which are attested as possible by any evidence we possess, are capable of causing a glacial age, while the same remark applies also to the dependent meteorological agencies, and we may put them aside, therefore, as impotent and irrelevant. If we are to find an efficient cause for an ice age, therefore, we must go outside normal conditions and factors and import abnormal ones into the problem. We must, in fact, postulate some cause not based on uniformity of action during all time, but one dependent on irregular and spasmodic action. Several such causes have been appealed to, and most of them have been criticised in my former work. It is my duty to summon them again before the only jury which tries such cases, and there to test their pretensions.

In his *Island Life* Dr. A. Wallace devotes a considerable space to a discussion of the causes of glacial epochs, in which he champions a modified view of Croll's theory. This I

ventured to criticise in my former work (*Glacial Nightmare*, pp. 417-421), and I shall now address myself to it at somewhat greater length.

Wallace places the glacial epoch between 240,000 and 80,000 years ago, with its culmination about 200,000 years ago, and makes it last 160,000 years, figures which must startle most geologists. During this period, he says, the greatest distance of the sun in winter was 98,250,000 miles, whereas now it is only 91,500,000, and inasmuch as the quantity of heat received from the sun is inversely as the square of the distance, this would be, he says, in the proportion of 9,613 and 8,372, or nearly one-seventh less than now (*op. cit.*, 2nd edit., p. 130). Here we have an ambiguity; the facts are true of *midwinter day*, but as a matter of fact the amount of heat received by the earth during a year from the sun is inversely as the length of the minor axis of the orbit, and, as we have seen, the earth as a whole was receiving more heat annually than it does now, and not less, whenever the eccentricity of its orbit was greater, and each hemisphere was doing the same.

In his first edition Wallace argued that in the glacial period the northern hemisphere had its mean winter temperature lowered by 35° F., and our northern summer had its heat enhanced by 60°, figures to which Croll took exception. Without giving us any word of explanation, these figures have in the second edition been altered to 39° and 48° respectively, which takes away much of Croll's objection to this particular paragraph.

Wallace then argued that while cold can be stored up in the shape of ice and snow during winter in high latitudes, it is not possible to store up heat in the tropics in the same way, since but little of the heat is actually absorbed by the ground for any length of time. A good deal of what it absorbs is radiated again and absorbed by the air or passes away into space, and the greater part of the tropical sun-heat is really carried off by convection currents of air and water. Hence Wallace argues that though heat cannot be stored up cold can, and that consequently a cumulative glacial age can be explained. But this argument involves a patent meteorological fallacy. The warm air which carries away the sun's heat of the tropics is a great evaporator and carries off immense

quantities of moisture. This moisture falls in summer in the shape of warm rain, and this rain and the warm ocean currents that also move polewards, if they do not accumulate or keep their heat, are engaged in mitigating the temperature and in a large measure melting the effect of the previous winter's cold in the shape of ice and snow, and do it most effectually, far more effectually than the direct sun-heat, and most completely prevent, under present conditions, any cumulative increase of the ice and snow. All this is completely overlooked by Dr. Wallace, who argues as if the direct sun-heat is the only thing to be considered.

Dr. Wallace accepts implicitly most of Croll's conclusions which we have examined and discarded as untenable. He accepts Croll's numerical results about the effects of a variable eccentricity with a winter in aphelion, which would according to him reduce the midwinter temperature of the northern hemisphere 36° F. and bring the January mean temperature of England and Scotland almost down to zero, or about 30° F. of frost, while the summer would be just as much hotter than it is now. He accepts his theory of the effects of enhanced eccentricity on the Trade winds and the consequent diversion of the great ocean currents, and also his theory about the intercepting effects of clouds and fogs. In fact he takes over almost all the machinery which Croll invented and employed, and which we have shown to be wanting in soundness, a frailty which naturally therefore affects his own results. In one important respect only does he venture to modify Croll's conclusion, or rather to add to Croll's postulates.

Croll, as we have seen, was baffled by his initial steps. If we could grant these, we might grant a good deal more. What he had to find was some cause which would tend to *accumulate* snow and ice, and not merely to make it. Without an accumulation of snow and ice in the first instance his machinery would not work. He thought he could explain and account for such an initial accumulation by merely invoking a colder and longer winter in aphelion in the time of greatest eccentricity without postulating any changes of any other kind than those which are quite normal, either astronomical or meteorological. To use his own words :

" I maintain that with the present distribution of land and water, without calling in the aid of any other geographical agencies than now obtain, those physical agencies detailed in *Climate and Time* are perfectly sufficient to account for all the phenomena of the glacial period, including those inter-related warm periods, during which Greenland would probably be free from ice and the arctic regions enjoying a mild climate ". Mr. Wallace, on the other hand, maintains that without assuming some change in the geographical conditions of our globe these physical agencies will not account for that state of things, at least in so far as the accumulation and disappearance of the ice in arctic regions is concerned. We have seen how futile Croll's boast was, and have seen that its futility was largely due to his being unable to start his machinery with an adequate store of cold in the shape of accumulated snow.

Mr. Wallace, who accepted the greater part of Croll's argument, put his finger upon this difficulty many years ago, and he has continually maintained that in order to start Croll's machinery we must postulate some factor which would cause an initial deposit of snow and ice. This factor he finds in certain geographical changes. He maintains continually that it is only when the higher latitudes were already snow-clad, owing to peculiar conditions not now prevalent, that the combined astronomical and meteorological agencies invoked by Croll would intensify them and cause a glacial period, and he also traverses another of Croll's most prominent positions, namely, that the glacial epoch of one hemisphere was represented by a glacial epoch in the other.

The changes in geographical conditions demanded by Wallace are apparently of two kinds. He lays it down that high land and great moisture " are essential to the initiation of a glacial epoch ". He also postulates such geographical changes as would interfere with and block the flow of the great ocean currents. Now, in regard to the former of these conditions, it does not seem to be available. As we shall show presently, there is no good evidence that during the so-called glacial period the land in the higher latitudes of either hemisphere was higher than it is now, but there is almost conclusive evidence that it was much lower. This

issue, however, I shall treat of at greater length in a later chapter. Let us now turn to Wallace's second condition.

In demanding a change of geographical conditions of this kind, Wallace was hampered by the views he holds in regard to the permanence of the great ocean and continental areas. Thus he says " there is the strongest cumulative evidence, almost amounting to demonstration, that for all known geological periods our continents and oceans have occupied the same general position they do now, and that no such radical changes in the distribution of sea and land as imagined by way of hypothesis by Sir Charles Lyall have ever occurred ".(*Island Life*, p. 144).

While he therefore postulates certain geographical changes as probable, or rather necessary, he has limited his choice very much. The most important change he actually appeals to is a breach in the Isthmus of Panama, which he argues would cause a partial divergence of the Gulf Stream. " A subsidence of the Isthmus of Panama has possibly happened more than once in Tertiary times. If this subsidence were considerable," says Wallace, " it would have allowed much of the accumulated warm water which initiates the Gulf Stream to pass into the Pacific ; and if this occurred while astronomical causes were tending to bring about a cold period in the northern hemisphere, the resulting glaciation might be exceptionally severe. The effect of this change would however be neutralised if at the same epoch the lesser and greater Antilles formed a connected land " (*op. cit.*, p. 145).

The question, however, is not whether the Isthmus of Panama has been broken through in far off tertiary times, —that may well be ; but whether it has been broken through during or since the distribution of the drift. On this question Prof. Geikie speaks very sensibly : " In order," he says, " to deflect the equatorial current into the Pacific, not only the Isthmus of Panama but a considerable portion of Central America would need to be deeply submerged. It may even be doubted whether in the event of such submergence taking place any considerable proportion of the equatorial current would flow into the Pacific. It seems just as probable that the counter current of the Pacific would escape into the Caribbean Sea, and thus increase the body of the equatorial

current." " There is no proof," he adds, "of any such disconnection of the two Americas having taken place in Pleistocene times, and there are no facts forthcoming to show that the Isthmus of Panama was contemporaneously drowned to such an extent as to allow the passage across it of the equatorial current" (*Supposed Causes of the Glacial Epoch,* Edin. Geol. Soc., 1891, p. 222). The best proof that the isthmus has not recently been breached is the complete divergence in the fish and molluscs of the rivers of the Atlantic and Pacific slopes respectively. A second proof is that in pleistocene times there must have been a land-bridge here to enable the mastodons, mylodons, etc., to pass to and fro between North and South America. But suppose we grant that there has been a breach in the Panama Isthmus since pliocene times, its effect in regard to the Gulf Stream would have been neutralised by another fact. There is a good deal of evidence pointing to the Antilles and the peninsula of Florida having then formed a continuous land barrier parallel to the Isthmus of Panama (*vide Glacial Nightmare,* 360-366)—the very antidote to his prime cause suggested by Mr. Wallace. We may therefore put this particular postulate aside as ineffective. In addition to this breach in the Panama Isthmus, he also suggests as possible that the British Islands may have become continental by being joined to the Faroes, Iceland and Greenland, which would prevent the Gulf Stream from having access to northern Europe, " and the result," he says, " would almost certainly be that snow would accumulate on the high mountains of Scandinavia till they became glaciated to as great an extent as Greenland; and the cold would react on our own country and cover the Grampians with perpetual snow, like mountains of the same height at even a lower latitude in South America "

Now it seems to me that if we want to get outside of hypotheses and to secure a perfectly good test of what the climate of north-western Europe was when these very continental conditions last prevailed, we cannot do better than examine the flora now prevailing in Iceland, the Faroes, the Orkneys and the Shetlands, and also the remains preserved in the bogs in these archipelagoes and islands. The flora must have been distributed over these islands when the land

communication was complete among them, and yet they give us no such answer as is demanded by Dr. Wallace's hypothesis. If the union of the various islands he mentions would have the effect, as he claims, of smothering the Grampians and Scotland generally with ice, how did the flora and fauna of all these scattered islands survive the catastrophe?

Apart from this it is not improbable that the course of the Gulf Stream has been considerably altered in recent times, as we shall see in a later chapter. But suppose it were entirely diverted it would no doubt cause a considerably more severe climate in western Europe, but we have no reason to suppose that it would in any way increase the permanent snow except on high mountains. It would doubtless introduce a climate into western Europe very like that which prevails on the same latitudes in eastern America, where the Gulf Stream hardly tells as a factor, and cause more sharply contrasted summers and winters instead of a greater mean temperature. But this is a very long way from introducing a glacial climate. We must always remember that with the disappearance of the Gulf Stream there would largely depart our warm south-west winds with their loads of vapour, and leave us north and north-east winds, cold and dry and very bad providers of snow. The evidence of the so-called glacial shells amply confirms the argument here used. It seems very probable, as we shall presently see, that in the time when the drift was distributed the isothermals of western Europe were diverted considerably to the south of their present position, and that its climate was more like the climate of similar latitudes in eastern America at this moment. This seems to me to be the extreme effect of the only diversion of the Gulf Stream which is attested by evidence, and its complete inadequacy to the demands put upon it by Dr. Wallace is obvious. Has there been any change then elsewhere? Dr. Wallace hints that if the east side of Greenland were to sink considerably, while on the west the sea bottom were to rise in Davis Strait so as to unite Greenland with Baffin's Land, it would stop the cold arctic current with its enormous stream of icebergs from the west coast of Greenland, and that such a change might cause a great accumulation of ice in the higher polar latitudes. But it would certainly produce a wonderful amelio-

rating effect on the climate of the east coast of America, and would raise the temperature of Labrador to that of Scotland. Hence, again, the argument, in so far as it illustrates the condition of things in so-called glacial times, is quite futile, since we know that instead of having been milder than now the climate of Canada was the reverse. The present range of arctic shells in North America, when compared with their range when the drift beds were deposited, shows that the isotherms there were diverted to the south at that time, just as they were in Britain.

Apart from this, as we shall see presently, there is no evidence of the district surrounding Greenland having been higher in the so-called glacial age, but the very opposite, so that the hypothesis in question is more than usually arbitrary and baseless.

It would seem therefore that Wallace's modification of Croll's theory offers us very small comfort if we are in search of a cause for an ice age, and that when analysed it proves to be as impotent as that theory is. This is not all however. Wallace, in common with a great number of geologists, will have nothing to say to the genial interglacial periods of Croll. With him, again, glacial conditions occurred together in both hemispheres, and merely meant the intensifying of a severe climate which already prevailed and without which no glacial period could be initiated.

To quote his own words : " The alternate phases of precession, causing the winter of each hemisphere to be in aphelion and *perihelion* each 10,500 years, would produce a complete change of climate only when a country was *partially* snow-clad, while wherever a large area became almost wholly buried in snow and ice, as was certainly the case with northern Europe during the glacial epoch, then the glacial conditions would be continued, and perhaps even intensified, when the sun approached nearest to the earth in winter, instead of there being at the time, as Mr. Croll maintains, an almost perpetual spring " (*Island Life*, p. 503). Again : " When geographical conditions and eccentricity combine to produce a severe glacial epoch, the changing phases of precession have very little, if any, effect on the character of the climate, as mild or glacial, though they may modify the season ; but when the eccentricity becomes

moderate and the resulting climate less severe, then the changing phases of precession bring about a considerable alteration and even a partial reversal of the climate " (*ibid.*, p. 153).

Again he says : " It follows that towards the equatorial limits of a glaciated country alternations of climate may occur during a period of high eccentricity, while near the pole, where the whole country is completely ice-clad, no amelioration may take place. Exactly the same thing will occur inversely with mild arctic climates " (*ibid.*, p. 154). Thus, as Croll says : According to Mr. Wallace there could be no warm interglacial periods, either in temperate or polar regions, except during the commencement and towards the close of a glacial epoch ; but this, when taken with his other arguments, involves a curious contradiction. If the arguments and contentions of Wallace are sound, if the ice and snow is to continue to accumulate at either pole in spite either of cool or of hot summers, or of a greater or a less eccentricity, I cannot understand how such a glacial period could ever have come to an end anywhere. On the contrary, it seems to follow necessarily that it ought to have gone on increasing in intensity and be culminating now, for the physical causes had *ex hypothesi* a cumulative force. This must suffice as a test of Dr. Wallace's theory.

Dr. Penck, who is the standard-bearer of the extreme glacialists in Germany, has adopted an attitude not unlike the modification of Croll's theory by Wallace (*Die Vergletscherung der Deutschen Alpen*, xxix., p. 444, etc.). The points of difference between him and Croll are chiefly three.

First, he apparently quite differs from him about the cause of the belt of calms and the reason why it moves north and south. In regard to the fact that this belt of calms is now situated more to the north than it is to the south of the equator, he also disagrees with the view that it is due to the considerable preponderance of land over water in the northern hemisphere, which is the view generally held, and argues that it is rather due to the fact of the sun being six days longer above the horizon in the northern than it is in the southern hemisphere, thus agreeing with Pilar. And he argues that 10,500 years ago, when by the operation of pre-

cession the conditions were reversed, the mean position of
the calms was as much to the south as it now is to the
north of the equator. Penck therefore disputes Croll's view
in regard to the cause of the shifting of the belt of calms,
and while attributing large results to the operation of ocean
currents and winds, he makes these the direct and not the
indirect effects of modified eccentricity as Croll does.

Although it is not of importance for our polemic, it seems
clear that Pilar and Penck are mistaken in supposing that
the preponderance of land in the northern hemisphere has
not a considerable influence upon *the position* of the belt of
calms. That position has only been defined with precision
in these latter years, and is not so uniform in regard to
latitude as it was supposed, but largely follows the distribu-
tion of land and water in the equatorial belt. I shall revert
to this presently.

The main point in which Penck differs from Croll is in
insisting upon the necessity of special local conditions to
produce an ice age. With him extreme eccentricity with
winter in aphelion will, when contrasted with the opposite
state of things, produce an alternation of cold and warm
climates. But these conditions will not produce an ice age
or the reverse unless we also have high lands to act as con-
densers of the evaporated vapour, and these high lands must
be near the seaboard. He urges that at present, glaciers
only exist within reach of maritime conditions and not in
continental districts, and so he contends it must have been
in the glacial age. But, as we have seen, and as we shall show
again, there is every reason to believe that the lands in high
latitudes in the northern hemisphere were for the most part
lower in so-called glacial times and not higher than they are
now, and Penck's modification of Croll's theory, therefore,
which is much too largely based upon *a priori* rather than
empirical evidence, is quite incompetent to solve our problem,
and rather increases than diminishes our difficulties, for the
only factor it adds to Croll's is one unsupported by the evidence.

While Wallace and Penck have combined certain geogra-
phical changes with a varying eccentricity in the earth's
orbit, Belt similarly tried to combine such a change with a
great enhancement in the obliquity of the earth's axis (see

Glacial Nightmare, pp. 364, 365). We have seen in the first chapter that Colonel Drayson had boldly postulated such a change in the teeth of all that the astronomers had had to say on the subject. He did not profess to explain it or to give any cause for it, and he boldly said that whatever the cause the path actually traced by the earth's pole, as marked out by the observations of the last 200 years, justifies his conclusion.

Mr. Belt was more philosophical. He grants that the great geometricians could not have erred in their calculations, but he proceeds to inquire whether they had had all the facts before them which we know now. He says : " They assumed in their examination of the problem that the thickening of matter around the equator was a constant quantity, whereas there are evidences of great upheaval and depression in remote ages that may have altered the conditions of the question. The gradual heaping up of ice around the poles in the glacial period must have in some measure diminished the difference between the polar and the equatorial diameters. Many physicists believe that even now an elevation of land around the poles and a depression of land in the tropics is taking place."

" The protuberance," he urges, " around the equator is not a regular one, but the equatorial circumference approaches in general outline to an ellipse of which the greater diameter is two miles longer than the other.[1] At the time the above-mentioned calculations were made the data did not exist for determining the irregularity. To the non-astronomical mind it appears evident that this great difference in the equatorial diameters is an element of great importance in the calculations, and as it was not considered we cannot admit that the problem has yet been decided. The great preponderance of land in our hemisphere, not arranged around the pole of the earth, but in a mass whose centre is situated near the English Channel, must also be a disturbing element of no mean importance " (*Quarterly Journal of Science*, ii., p. 443).

All this sounds very plausible so long as we are dealing

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[1] This view is based on the geodetic results obtained by Colonel Clarke, but more recent researches have greatly reduced this disparity, and perhaps even done away with it altogether.

with wonderland, but when we come down to the actual facts and apply the rigid methods of the mathematician to them, they at once dissipate into thin air. This has been done in the present instance by, amongst others, the very competent pen of Prof. George Darwin. He has shown that " supposing the whole equatorial regions up to latitude 45° north and south were sea, and the water to the depth of 2,000 feet were placed on the polar regions in the form of ice, and this is the most favourable redistribution of weight possible for producing a change of obliquity, it would not shift the arctic circle by so much as an inch " (Croll, *Discussions on Climatology*, p. 4).

. "It has been demonstrated," says Mr. James Geikie, " that the protuberance of the earth at the equator so vastly exceeds that of any possible elevation of mountain masses between the equator and the poles, that any slight change which may have resulted from such geological causes could have had only an infinitesimal effect upon the general climate of the globe" (*Great Ice Age*, 3rd edit., p. 790).

This is surely absolutely conclusive in regard to the possibility of any geographical changes having affected the range in the obliquity of the earth's axis. But apart from this Croll, with his usual clearness, has shown how completely impossible it is for any increase in this obliquity to cause a glacial age, and has accordingly put out of court most effectively all such appeals as those of Drayson, Belt, and others. Thus he says : " The polar regions owe their cold not to the obliquity of the ecliptic, but to their distance from the equator. Increased obliquity, in fact, tempers their climate, it diminishes the heat received in the equatorial regions, and increases that about the poles. With the present obliquity the proportion is 100 to 42·47. If the obliquity were increased to 35, the amount at the equator would be reduced to 94·93 parts, and that at the poles increased to 59·81, being an increase at the poles of nearly one half. At latitude 60° the present quantity is 57 parts, but this would increase to 63 under similar conditions. If the arctic circle were brought down to England, the temperature of these islands would be enhanced and not diminished. The winters would be colder but the summers would be hotter in a much greater degree, and the greater the obliquity the more would the increase of summer

heat exceed its decrease in winter. If the obliquity were increased until it reached its absolute limit of 90°, when the earth's axis would coincide with the plane of the ecliptic, the arctic circle would then extend to the equator. Would this produce a glacial period? A square foot at the pole would receive as much heat as a square foot at the equator if the sun remained on the equator the entire year, and the total amount received would be as the ratio of half the circumference of a circle to the diameter, *i.e.*, 1·5708 to 1, or half as much heat again; and all this heat received by the poles would be concentrated into six months, so that they would be receiving twice as much heat as is received by the equator at present in the same time, and more than three times what it received then. There would be continuous sunshine there for six months without the ground having the opportunity of cooling for a single hour. Nothing living on the earth could exist in the polar regions under so fearful a temperature as would then prevail in the summer months. How absurd to suppose that this would tend to produce a glacial epoch. Not only would every particle of ice in the polar regions be dissipated, but the very seas around the pole would be at boiling point."

It is thus perfectly plain not only that any postulated change in the obliquity of the earth's axis in excess of the very small range admitted by Lagrange and others is quite out of the question, but also that any such change, even if it were admissible, would have the opposite effect to that suggested by its sponsors, and be quite incompetent to create an ice age.

Let us now turn to another supposed change in the earth's axis, which I criticised at some length in my previous work (*Glacial Nightmare*, pp. 341-354). The theory in question postulates the possibility of a more or less secular motion of the earth's axis within the earth itself, by which the two poles would have a different emplacement on the periphery of the earth, and the equator and all parallels of latitude would be correspondingly shifted. The view in question has recently been revived by my friend Mr. Oldham in order to explain supposed glacial conditions in the tropics in Permian times.

It seems also to have occurred to Dr. Nansen, who, apparently quite unconscious of the difficulties of such a solution, which have been pointed out by many critics, jauntily says : " The easiest method of explaining a glacial epoch, as well as the occurrence of warmer climates in one latitude or another, is to imagine a slight change in the geographical position of the earth's axis. If, for instance, we could move the North Pole down to some point near the west coast of Greenland, between 60° and 65° north latitude, we could, no doubt, produce a glacial period both in Europe and America " (*The First Crossing of Greenland*, ii., p. 454). The recrudescence of this view makes it necessary to add a few words by way of argument to those already adduced in my former work.

In a paper published by Mr. Oldham in the *Geological Magazine* for 1886, p. 307, he urges that there is evidence of a change of latitude having occurred in recent times in some of the chief European observatories, and he quotes from a paper of Prof. Asaph Hall, published in the *American Journal of Science* for March, 1885, a series of facts there given proving this change of latitude, and which he thus tabulates :—

Washington,	1845	.	.	.	.	. 38° 53′ 39″ 25
,,	1863	.	.	.	.	. 38″ 78
Paris,	1825	.	.	.	.	. 48° 50′ 13″ 0
,,	1853	.	.	.	.	. 11″ 2
Milan,	1811	.	.	.	.	. 45° 27′ 60″ 7
,,	1871	.	.	.	.	. 59″ 19
Rome,	1810	.	.	.	.	. 41° 53′ 54″ 26
,,	1866	.	.	.	.	. 54″ 09
Naples,	1820	.	.	.	.	. 40° 51′ 46″ 63
,,	1871	.	.	.	.	. 45″ 71
Königsberg,	1820	.	.	.	.	. 54° 42′ 50″ 71
,,	1843	.	.	.	.	. 50″ 56
Greenwich,	1838	.	.	.	.	. 51° 28′ 38″ 43
,,	1845	.	.	.	.	. 38″ 17
,,	1856	.	.	.	.	. 37″ 92
Pulkowa,	1843	.	.	.	.	. 59° 46′ 18″ 73 ± 0″ 013
,,	1866	.	.	.	.	. 18″ 65 ± 0″ 014
,,	1872	.	.	.	.	. 18″ 50 ± 0″ 014

Mr. Oldham candidly admits that too much cannot be made of these figures since they cover such a short period, but he says it is significant that they all tend in one direction.

They prove that the earth's axis is not fixed absolutely in its present position, but they prove very little more. They have been subjected to a searching examination at the hands of Mr. Chandler (*Astronomical Journal*, Boston, Mass., 1892, xii., pp. 57-62, 65-72, 97-161). The question is a difficult one, for the amount of variation is very slight, and the measurements very minute. Chandler has shown, however, that the variations of latitude occur in two cycles, one of twelve and the other of fourteen months, with no appreciable secular change. Sir Robert Ball tells us that if a circle be drawn in a certain part of the arctic region with a radius of thirty feet the pole of the earth will constantly be found within the circumference of this circle. The period during which the pole traverses this circle is about 427 days, and the place of the pole, since the glacial period and from even earlier geological time, is believed to have been without greater changes than would be inside the area of a block or square enclosed by the intersecting streets of a city (*Fortnightly Review*, 1893, pp. 171-183).

It is clear, therefore, that the shifting of the earth's axis, as suggested by Mr. Oldham and others, while possible, is, under normal conditions, limited to a very slight amount, so slight as to be inappreciable. In order that it should become appreciable the centre of gravity of the earth must be moved to a considerable extent also, and this seems to be quite an impossible contingency. Let us see.

Sir A. Geikie, speaking of this view, says : " If it can be shown that the geographical revolutions necessary to shift the axis are incredibly stupendous in amount, improbable in their distribution, and completely at variance with geological evidence, we may reasonably withhold our belief from this alleged cause of the changes of climate during geological history ".

It has been estimated by Sir William Thomson " that an elevation of 600 feet over a tract of the earth's surface 1,000 miles square and ten miles in thickness would only alter the position of the principle axis by one-third of a second, or thirty-four feet. Mr. George Darwin has shown that, on the supposition of the earth's complete rigidity, no redistribution of matter in new continents could ever shift

the pole from its primitive position more than 3°, but that if its degree of rigidity is consistent with a periodical readjustment to a new form of equilibrium, the pole may have wandered some 10° or 15° from its primitive position, or have made a smaller excursion and returned to near its old place. In order, however, that these maximum effects should be produced, it would be necessary that each elevated area should have an area of depression corresponding in size and diametrically opposite to it, that they should be on the same complete meridian, and that they should both be situated in latitude 45°. With all these coincident favourable circumstances an effective elevation of $\frac{1}{300}$ of the earth's surface to the extent of 10,000 feet would shift the pole 11$\frac{1}{2}'$; a similar elevation of $\frac{1}{20}$ would move it 1° 46$\frac{1}{2}'$; of $\frac{1}{10}$ 3° 17'; and of $\frac{1}{4}$ 8° 4$\frac{1}{2}'$. Mr. Darwin admits these to be superior limits to what is possible, and that, on the supposition of intumescence or contraction under the regions in question, the deflexion of the pole must be reduced to a quite insignificant amount. Under the most favourable conditions, therefore, the possible amount of deviation of the pole from its first position would appear to have been too small to have seriously influenced the climates of the globe within geological history. If we grant that these changes are cumulative, and that the superior limit of deflexion was reached only after a long series of concurrent elevations and depressions, we must suppose that no movements took place elsewhere to counteract the effect of those about latitude 45° in the two hemispheres. But this is hardly credible. A glance at a geographical globe suffices to show how large a mass of land exists now both to the north and south of the latitude, especially in the northern hemisphere, and that the deepest parts of the ocean are not antipodal to the greatest heights of the land. These features of the earth's surface are of old standing. There seems indeed to be no geological evidence in favour of any such geographical change as could have produced even the comparatively small displacement of the axis considered possible by Mr. Darwin."

Lastly, the late Prof. Haughton says : " The question of how much change on the poles could be produced in this way has been attempted by Mr. George Darwin and myself. Mr.

G. Darwin takes the area of Pacific depression as estimated in his father's researches on coral reefs, and determines mathematically the form and position it should have in order to produce the maximum of change of position of the poles, and finds the maximum change of latitude to be 3° or 210 miles. I took the case of Europe, which we know has been raised since the commencement of Tertiary times, and calculated mathematically the change of latitude which was caused by the elevation of that continent, and found it to amount to only 1° or seventy miles. This, of course, postulates changes of geography, since the so-called glacial age quite unsupported by any evidence, and yet the results are quite inappreciable as causes of a glacial period."

Apart from the suggestion of a substantive movement of the earth's axis within the body of the earth, which, as above shown, is not a cause available for the explanation of an ice age, Mr. Oldham has also revived the notion first propagated by Sir J. Evans of a possible movement of the earth's crust over a central more or less fluid nucleus. This view has also been favoured by M. Jules Peroche in his memoir entitled *Les végétations fossiles* (*Méms. de la Société d'Archéologie et d'Hist. Nat. de la Manche*, vii., 1886), who has added some further notions to it. According to this last writer, the movement of the solid envelope over the postulated liquid nucleus is due to the same forces which cause the movement of precession and acts most powerfully on the equatorial region, and causes the shell to move more slowly than the nucleus. He thus accounts for a motion of the poles over a range of 13°. Thus all points on the earth would approach the poles or the equator to a distance of 30°. This movement, he says, is very slow, and covered by a period of 1,200,000 to 1,300,000 years. He further urges that in the two polar areas the crust of the earth is much thicker than at the equator, from its having cooled faster and from the polar diameter being less, and therefore there being a smaller reservoir of heat. I quote the greater part of this merely to show how supremely fantastic deductive science may become. The only part I wish to seriously criticise, and which I have criticised already in my former work, is the hypothesis of the earth consisting of a solid shell over a liquid nucleus and what results from it.

The condition of the interior of the earth still remains more or less a crux to mathematicians and physicists. The elementary phenomena of volcanoes, hot springs, etc., and the known fact that wherever we choose to dig into the ground the temperature increases steadily as we go down prove that the earth's interior is hot. The increase last named is at the rate of about 1° F. for every fifty feet. Hence it has been rationally concluded that the surface of the earth was once at a high temperature and probably molten. Suppose such a red hot liquid globe was to begin to cool like lava cools. When any portion of it was cooled to a certain temperature it would congeal and become solid. If the matter thus solidified was lighter than the liquid substratum it would float on it like a crust, if it was heavier it would sink and be replaced by other layers of liquid matter, and eventually, according to Hopkins and Thompson, a crust would be built up of a cancellous structure containing great hollows in which liquid lava would remain.

Lord Kelvin has shown that the temperature of the earth does not necessarily increase uniformly as we go down mile after mile, but that the rate of increase probably diminishes rapidly when we get down to considerable depths, that at 800,000 feet deep the rate of augmentation reaches zero, and below this point underground temperature does not sensibly increase at all. " I do not presume," says Lord Kelvin, " to fix within any limits even of rough approximation what the greatest temperature reached in going downwards may be. It may be less than 4,000° or 5,000° F. It may possibly be as much as 8,000° or 10,000°, but there is a vast difference between 5,000° or 10,000° and the 100,000° or 1,000,000° which we frequently find in geological disquisitions." Hence he concludes we must give up the argument derived from the phenomena of underground temperature as to the internal fluidity of the earth. They show that the earth is at a high temperature, but not at any temperature so high as to make general rigidity of the earth's interior impossible or improbable.

This enables us to face the question of the interior condition of the earth as deduced from astronomical and other considerations with more facility. These arguments are based

on two considerations, namely, on the phenomena of precession and nutation, and, secondly, on the phenomena of the tides. Hopkins in working out the conditions of precession and nutation found that in order that the calculated periods and the actual results should equate, the earth must be considered as on the whole rigid (*Phil. Trans.*, 1839, pp. 40, 41 *passim*). Delaunay in a paper on the hypothesis of the internal fluidity of the terrestrial globe (*Acad. des Sciences,* 13th July, 1868) contested the view of Hopkins and concluded that the phenomena of precession and nutation cannot furnish us with any data whatsoever relative to the greater or less thickness of the solid external crust of the globe (see also *Geol. Mag.*, November, 1868).

It would seem that Delaunay's arguments are sound, for Lord Kelvin has apparently given up an older opinion of his own on this subject, and now says that the arguments derived from the phenomena of precession and nutation present considerable difficulties, and indeed do not afford us at the present time a decisive answer. " The phenomena, however, of the tides," he says, " lead us to no uncertain conclusion. Suppose the earth to consist of a thin shell or crust existing or floating on a vast interior of molten matter. The liquid interior would tend to yield freely to the tide-generating influence of the sun and moon. The consequence would be that the interior crust would be acted on by forces which, unless it were of preternaturally rigid material, it would be unable to resist. The crust would then be subject to upheavals and depressions taking place in time with the revolutions of the sun and moon. If the crust yielded *perfectly* there would be no tides of the sea, no rising and falling relatively to the land at all. The water would go up and down with the land, and there would be no relative movement, and in proportion as the crust is less or more rigid the tides would be more or less diminished in magnitude. Now we cannot consider the earth to be absolutely rigid and unyielding. No material that we know of is so. But I find from calculation that were the earth *as a whole* not more rigid than a similar globe of steel, the relative rise and fall of the water in the tides would be only two-thirds of that which it would be were the rigidity perfect ; while if the rigidity were no greater than a

globe of glass, the relative rise and fall would be only two-fifths of that on a perfectly rigid globe.

" Imperfect as the comparison between theory and observation as to the actual height of the tides has been hitherto, it is scarcely possible to believe that the height is only two-fifths of what it would be if, as has been universally assumed in tidal theories, the earth was perfectly rigid. It seems, therefore, nearly certain, with no other evidence than is afforded by the tides, that the tidal effective rigidity of the earth must be greater than that of glass. This is the result taking the earth as a globe uniformly rigid throughout. That a crust fifty or a hundred miles thick would possess such preternatural rigidity as to give to the mass, part solid and part liquid, a rigidity as a whole equal to that of glass or steel is incredible, and we are forced to the conclusion that the earth is not a mere thin shell filled with fluid but is on the whole in great part solid " (Lord Kelvin, *Popular Lectures and Addresses*, ii., pp. 299-318).

The question of the rigidity of the earth has since this was written continued to be fought over, and notably by two accomplished champions, the Rev. O. Fisher and Mr. George F. Becker. Some of Mr. Becker's conclusions are interesting, and I will venture to quote one or two. He uses the phrase " rigidity of the earth " with the meaning of " the earth's resistance to change of shape ". His conclusions based on other methods of calculation confirm those of Lord Kelvin and Prof. Darwin. He points out that if the earth were a dense fluid of density 5·5 and the ocean rested on it, both would yield to the attraction of the moon almost equally, and there would be no considerable apparent tide. The actual total amplitude of the tides at oceanic ports is usually from two to three feet, but these tides are greatly affected by the rotation of the earth, the obstruction of continents, and the inequalities of the bottom, and are not therefore equal to the equilibrium tides.

" If the globe were as rigid as glass, the apparent tides would be only $\frac{2}{5}$ of the real tides, or, in other words, perfect rigidity would then increase the apparent tides to $\frac{5}{2}$ of their present amplitude, so that the amplitude of the tides at oceanic stations would increase to from 5 to $8\frac{1}{2}$ feet, and would be much greater than the lunar equilibrium tides.

"If the rigidity were really that of brass, infinite rigidity would raise the oceanic tides to from 4 to 6 feet. If the earth were as strong as steel, these tides would be raised by perfect rigidity to from $2\frac{1}{2}$ to $4\frac{1}{4}$ feet. . . . If the earth had the rigidity of brass, the semi-diurnal rise and fall of the land would be half the amplitude of the real tides, and would equal the apparent tides. Were the continents uniform in lithological composition and elevation, the tidal wave might pass under our feet unnoticed, as it does under a ship at sea. But it seems to me that at points near abrupt changes of density, as along the foot of a great mountain range or near the edge of a great oceanic abyss, such as lies close to the Pacific coast, a semi-diurnal rise and fall of two or three feet would surely manifest itself in differential displacements.

"The earth is thus a very rigid body. . . . The average rigidity of the rocks exposed at the surface of the earth is certainly much less than that which a continuous mass of glass or brass would exhibit, and there is thus a distinct difficulty in accounting for so high a rigidity as the earth evinces, even on the theory that the globe is solid. On this point, however, certain experimental results of Dr. William Hallock are very instructive. He subjected wax and paraffin to a pressure of 96,000 pounds per square inch in a horizontal steel cylinder, and on the top of these substances he placed small silver coins. The coins were forced against the tube with such violence as to leave in the steel impressions which could be felt as well as seen. Thus substances which at one atmosphere show trifling rigidity may develop a rigidity comparable with that of steel at pressures such as are to be found at about fifteen miles below the surface of the earth" ("An Elementary Proof of the Earth's Rigidity," *Amer. Journ. of Science*, xxxix., May, 1890).

In a second paper, entitled "Fisher's New Hypothesis (*Amer. Journ. of Science*, xlvi., August, 1893), Mr. Becker concludes thus:—

On any theory yet propounded for the tides, the existence of semi-diurnal tides indicates an earth presenting great resistance to deformation. This resistance, so far as the tides are concerned, may be due either to rigidity or to the viscosity of an ultraviscous fluid, some 20,000 times as vis-

cous as hard brittle pitch at 34° F. Quoting Darwin, he comes to the conclusion "that no very considerable portion of the interior of the earth can even distantly approach the fluid state". This is the view, I believe, of nearly all the great physicists, and if it be true it altogether precludes the notion of a solid shell moving on a liquid nucleus. Apart from this it would seem very unlikely even if the actual centre of the earth is in a fluid state that there should be a sudden and complete division between it and the solid slag which forms the crust. It is much more probable that under the vast pressures concerned there is a gradual solidification from the point of greatest fluidity to the completely solid state, and this involves a second difficulty in conceiving how and where the crust can be said to definitely end and the nucleus to begin, so that any such shifting of the envelope over the nucleus would involve portentous friction.

This would put a great barrier in the way of the revolution of the external part of the earth over its own central portions. But the great difficulty of understanding how this movement could take place at all, as I showed in my former work, is due to the fact that the earth has a vast fly-wheel round its centre in the shape of an equatorial protuberance. How without great dislocations could this supposed shell, weighted in this way, shift the direction of its axis so that its axis of rotation and its axis of figure should be different when it is spinning round at the gigantic pace the earth spins? The whole idea, when thus tested, seems fantastic.

Sir John Evans tried to minimise these last difficulties by arguing that inasmuch as a considerable portion of the equatorial zone is occupied by water the actual bulge of the belt at that point must be less than is generally argued, and he suggests than we may in fact diminish it fairly by six miles. This seems a quite excessive amount of irregularity to postulate of the earth's shape, since the average depth of the ocean has been calculated to be 2,000 feet only, and a large part of the tropical part of the Atlantic is occupied by comparatively shallow banks. But allowing the full amount, it is still only a very slight unweighting of the fly-wheel, and it is only effective where there is water and not where there is land.

Sir John Evans, in addressing the Geological Society

(*Anniversary Address*, 1876, p. 62), framed his hypothesis in more concrete form, and put it into a shape in which it could be tested mathematically. Prof. Twisden subjected his premises and conclusions to a rigid analysis, and showed that, granting all Sir John Evans's data, instead of causing a change in the position of the pole of 15° or 20°, as he surmised, they would only account at the most for a movement of 10′, and that to obtain a movement of 1° the zone of land appealed to by Evans must be raised five miles, while to move the pole 20° it would require the removal of a sixth part of the equatorial bulge. Even if a transfer of this enormous amount of matter (for which we have not a tittle of evidence) took place, it would not necessarily cause any effect of any moment, and might only produce a small effect on the axis of figure (*Quart. Journ. Geol. Soc.*, xxxiv., p. 41). Croll puts a graphic case when he says that a continent ten times the size of Europe elevated two miles would do little more than bring London to the latitude of Edinburgh or Edinburgh to the latitude of London, and adds : " He must be a sanguine geologist indeed who can expect to account for the glaciation of this country or for the former absence of ice around the poles by this means ".

Apart from all these desperate difficulties which have been pointed out by all the astronomers and physicists of repute who have examined the possibility of the change of position of the earth's axis within the earth itself, and which make the hypothesis impossible, there is the further crucial fact to which I referred at length in my previous work, namely, the impossibility of so locating the new axis as to explain the facts of paleontology and the distribution of the so-called glacial phenomena. In my former work I showed at considerable length that these phenomena are disposed quite unsymmetrically. That if we take the northern hemisphere they only occur in one half of it, and that from the White Sea eastwards as far as the eastern borders of Alaska they are wanting, while the erratic boulders, instead of pointing to a general drift of an ice sheet from the pole outwards, point, on the contrary, in high latitudes to a general inflow towards the pole. All this is quite inconsistent with any theory depending on a change of position of the earth's axis as

an efficient cause of an ice age. As I also showed in my former work (*Glacial Nightmare*, pp. 350-354), the paleontological evidence is similarly conclusive that the pole has not been shifting about during geological time, but has been fixed where it is. I will here only add an additional proof.

In summing up his interesting papers on some problems of arctic geology, published in *Nature*, lvii., pp. 351, 352, my friend, Prof. J. W. Gregory, writes in regard to the shifting of the earth's pole. Neumayr has published a map of the probable climatic zones in the Jurassic period, which appear to have been as parallel to the equator then as they are now. In tertiary times the evidence of the fossil plants seems to show the same; for from whatever direction we approach the pole, the fossil flora becomes sparser and more boreal in aspect, as we may see by a comparison of the plants of Disco Island and Grinnell Land, of the Great Slave Lake and Prince Patrick Land, of Iceland and Spitzbergen, and of Saghalien and New Siberia.

Hence the paleontological evidence, instead of demanding the shifting of the pole, seems to be opposed to this theory, and to show that, in all the periods for which paleontological evidence is available, the pole stood near its present position.

It would seem, therefore, that a shifting terrestrial axis is not a promising basis for a glacial theory. The hypothesis is, however, so far as I know, extinct, and I do not know any one who now maintains it.

This completes, so far as I am aware, the list of theories which have appealed to the variation of the earth's movements, either about its axis or its orbit, as possible explanations of an ice age, and they all fail to meet the necessary conditions of mechanics, physics and astronomy, apart altogether from their failing to satisfy the geological conditions to which we shall presently turn.

Let us now turn to another set of hypotheses, namely, those dependent upon a variation in the amount of heat radiated from the sun. This source of possible climatic change has been put forward in several forms, some simple and some complex; and, among other people, by Prof. Bruckner, who, prudently however, fails to analyse its necessary conditions; but it is only by such an analysis that a theory is

permissible. Let us first turn to the notion that a variable climate has ensued from a variation in the spots on the sun. It has long been noticed that the sun is at one time apparently glowing from its centre to its circumference, and sometimes its sphere is marked by dark spots whose origin is still partially a mystery, but which apparently qualify more or less the fierce heat of the solar sphere. The variation in the number and size of the spots has been urged by several writers as a cause of an ice age. One of the first to suggest it was M. Renoir (*Bull. Soc. Géol.*, 1839, xi., p. 64; 1840, p. 499), who urged that an increase or decrease of the sun-spots might affect terrestrial climate sufficiently to cause an ice age. A similar view was maintained by Balfour Stewart, Leblanc, etc. Wolff, Buys-Ballot, and others, as I showed in my former work (p. 334), have contested this theory, but the matter had been already put to experimental test. In the *Uranographie* of Francœur, published in 1837, p. 90, we read : " L'existence des taches a paru, à quelques astronomes, concorder avec une saison chaude ; on cite qu'en 1823 l'été s'est trouvé froid et humide ; le thermomètre ne s'est élevé qu'à 23° 7′ Réaumur à Paris, et le soleil n'a montré aucune tache, tandis qu'en 1807 l'été a eu de grandes chaleurs, et les taches ont été fort étendues. D'autres personnes résistent à cette opinion, et pensent qu'il n'y a aucune relation entre ces circonstances. Des hivers très rigoureux, des étés très chauds sont arrivés en l'absence des taches ou en leur présence. L'an 1783 fut remarquable par sa fertilité et la grandeur des taches solaires ; un brouillard sec couvrit l'Europe, et fut suivi des tremblements de terre de la Calabre." " Vous le voyez donc bien," says M. Angelot, " il est impossible d'appuyer sur aucune observation précise, sérieuse, et surtout générale, l'idée du refroidissement momentané de la terre par suite d'un excessif développement des taches solaires, et de leur persistance." M. Angelot, in view of the eagerness of geologists to find a substantial basis for the *Glacial Nightmare*, has some sarcastic remarks on the failure of their appeal to sun-spots, which I cannot forbear quoting. He says of this appeal : " Elle a un grand avantage sur toutes les autres causes possibles ou impossibles. Les taches du soleil n'obligeant à

rien, on les a toujours à sa disposition. On peut les faire paraître et disparaître à volonté. Sans règles fixes, sans causes connues, elles offrent autant de ressources que les comètes, qui sont, comme chacun sait, la propriété exclusive non des astronomes, mais des géologues, pour expliquer les révolutions inexpliquées du globe. Ce sont donc de fort utiles auxiliaires qu'il serait dommage de perdre " (*Bull. Soc. Géol.*, xii., pp. 96, 101).

It is quite plain that while it has been shown that sun-spots obey a certain law of periodicity, we must conclude with Wolff (who had observed them for many years) that they so far as we can see exert no influence on the annual temperature of the earth.

M. Vicaire (*Bull. Soc. Géol.*, 1874, 3rd ser., ii., p. 211) suggests that the glacial period was due to the diminution in the sun's heat, due to the exhaustion of the combustible materials which it contains. But if this were so, we ought to have evidence in the paleontological record of the gradual refrigeration of the earth's climate, which we have not. How, again, are we to account for the earth having recovered its old temperature after the supposed glacial period. If that period was caused by the very rapid exhaustion of the sun's heat, the sun must have acquired a considerable fresh supply to enable glacial England to be succeeded by our merry England. This is apart from other more general considerations to be referred to presently.

A more elaborate theory of the same kind has been a good deal favoured in France. The advocates of this view accept the nebular theory of Laplace, according to which the sun is only the centre of an ancient nebula which has been continually condensing upon itself, and throwing off as it has done so the planets in succession.

When the earth had been thus thrown off, it is argued, the nebular nucleus, or rather the sun in a very nebulous condition, must have occupied a vast space, and its surface was consequently near that of the earth. By continuing the process of condensation, the sun grew gradually smaller, and its distance from the earth increased. Presently other planets were thrown off. The diameter of the orbit of these planets is the measure of the solar diameter when they detached

themselves. A result of this was, as M. Faye puts it, that the
earth was formed before the sun. Basing his arguments
on the process thus described, M. Blandet, who was
followed by Saporta and D'Archiac, wrote in 1868 (*Bull.
Soc. Géol.*, 2nd ser., xxv., p. 777) a memoir, in which he
argued that the solar effect on terrestrial climate de-
pends on the diameter for the time being of the shrinking
sun. With an apparent diameter of 47° the whole climatic
problem would according to this view be altered. The
radiating surface of the sun would be so large and the
cone of solar rays proceeding from it would so envelope
the earth that the difference between the seasons would
disappear; no part of the earth would be plunged in per-
petual night; latitude would cease to be a climatic factor;
the poles would enjoy a mild climate, while the nebulous
character of the solar substance would prevent the tropics
from being roasted, as their proximity to the sun might suggest.
On no part of the earth would there be a night of twenty-four
hours long, and oblique rays would be the exception. In
consequence of its nebulosity the heat received from the sun
would be less intense, but the sun being much bigger and
nearer to the earth, it would have a no less efficacious effect.

Such is the postulate—a very plausible postulate so long as
we are dealing with the problems of cosmogony in the dawn
of time, but surely quite fantastic when applied to explain
geological changes of climate, and particularly to explain a
glacial period. Yet M. Falsan, following M. Blandet and
Saporta, accepts it as the most reasonable theory yet
propounded to explain the uniformity of geological conditions
and of climates which apparently prevailed all over the world
until middle tertiary times, and the problem of how plants
apparently disappeared from the poles and emigrated to the
south. This was, we are told, because the sun, by continued
concentration, only supplied them with weak and oblique rays.
The climate of the poles is thus supposed to have gradually
changed; the difference between the seasons became accentu-
ated, while the vapour in the air supplied abundant snowfalls.
At one time neither at the poles nor on the mountains, which
were then much lower, were there conditions possible for the
formation of ice. With the first appearance of ice and snow on

the earth, there began the glacial period properly so called. This
new factor introduced new climatic conditions and latitudes,
the variable action of marine and atmospheric currents, sub-
terranean movements of the earth, etc., and all became
effective instruments of climatic change. Of these instru-
ments the raising of lofty chains of mountains to act as
condensers was the most effective (A. Falsan, *La période
glaciaire*, pp. 202-204). I have had something to say in re-
gard to this extraordinary theory in my former work, from
which I must quote a paragraph. "It is quite true, if
Laplace's nebular theory be sustained, that in the remote
æons, at the beginning of time, such a condition of the sun
as is here pictured may have existed. It is also true that
the earlier geological periods point to there having once
been a greater uniformity of climate than there is now; but
it is a long way from these statements to the conclusion
that in quite recent geological times the sun has shrunk in
this portentous way. Again, as M. S. Reinach says, how
are we by such a cause to explain how the glacial period ever
came to an end? Any theory which explains the beginning
of the glacial period must, if it be of any value, also explain its
close" (*op. cit.*, p. 341). Every geological fact we know goes to
show that both the sun and earth had long passed out of their
nebulous or semi-nebulous condition when geological time
began, and that in so-called glacial times at all events their
constitution was very much what it is now. Let us there-
fore pass on.

Eugen Dubois, in a paper entitled "Die Klimate der
Geologischen Vergangenheit und ihre Beziehung zur Ent-
wickelungsgeschichte der Sonne" (Leipzig, 1893), urges that
"the high temperatures which prevailed in tertiary times in
the arctic regions were not obtained at the expense of lower
latitudes; therefore he says the total heat received by the
earth must have been once greater than at present". Again,
he says "the glacial climates indicate that the earth has twice
received a less amount of heat than now, and these differences
must have been considerable to affect climate". The former
postulate, it will be remarked, is a tremendous assumption.
We know literally nothing that would warrant the conclusion
that the enhanced temperature of the polar regions in tertiary

times was not coincident with a diminished temperature in the tropics. Let that pass, however.

Dubois proceeds to account for the change of climate involved, by an appeal to the cooling of the sun, and he does not scruple to enter into details. He points out that stars can be divided into three classes: white stars, like Sirius and Regulus, which are intensely hot; yellow or sometimes orange stars, to which class Capilla and our sun belong, and which are cooler; and a third and cooler class again, namely, the red stars. He affirms that the white stars constitute 58·5, the yellow ones 33·5, and the red 8 per cent. of the whole of the known stars, and he infers that their proportions mark the phases of the total period of luminosity of the stars in general. Applying this reasoning to the sun, he holds that the white state represented three-fifths of its entire life as a star, but that we have no astronomical data by which to calculate how much of the yellow phase it has seen. With the dominance of the white phase he correlates the long age extending down to the end of the secondary period, during which warm conditions presided over the whole earth. Arguing from the scarcity of transition stars from white to yellow, he holds that the passage of the sun from the white to the yellow phase was rapid, and was marked on the earth by the introduction of the tertiary period with its fast diminishing temperature, and that the yellow phase was fully established at the beginning of the pleistocene, an epoch so recent as to be probably separated from us by only one-sixtieth of the time which separates us from paleozoic times; whence he argues the sun has only recently entered into the yellow stage, and that three-fifths of its existence as a light and heat giving body has been passed through, and he argues that the glacial epoch with its supposed oscillations between glacial and interglacial climates corresponds to oscillations in the sun between its yellow and its red phases.

" Helmholtz," says Dubois, " has shown that the sun draws almost its entire energy from the contraction of its mass, and he and Kelvin assign 20,000,000 years for the past duration of the sun's heat, and argue that the sun has parted with 3·3 to 4 times as much heat as it still retains." He urges that these figures do not quite accord with the proportionate duration

indicated by the classification of the stars, and it may be necessary, he says, to limit the past duration to 10,000,000 years, and to assume that the radiation in the white state was twice as intense as at present.

He compares the earth when the sun was in its white phase to Venus, which receives twice as much solar heat as the earth, and is shrouded in thick clouds. Such a cloud covering, he urges, must equalise the distribution of heat, since it acts as a great intercepter of the solar rays.

He attributes the glacial period to very important depreciations in solar radiation having occurred at that time. He holds that periodical variations of short duration occur with stars of the yellow class, and that many variable stars belong to the transition group and to the third class, while our sun is subject to changes of a short period, one of which of eleven years is indicated by sun-spot phenomena.

Again, he holds that the interglacial periods were longer than the glacial. They were nevertheless short-lived, each one however longer than the last preceding glacial age. I have referred to this theory at some length as a specimen of modes of thought and argument which still survive among us, and which seem likely to outlive all the teachings of Bacon. Those who have tried to unravel the skein of paleontological and geological development and progress have all felt that a vast vista of time is a necessary factor in the problem; not only a vast vista of time but of conditions prevailing during most of that time which are not far removed from those now prevailing. The conditions of animal and vegetable life as we know them are limited by certain narrow bounds. To import into our problem the mighty cosmic changes involved in the progression of the sun from the condition of a star of one colour to that of another, and to postulate this as an explanation of an ice age, an ice age which was not the culmination of a long succession of changes all in the direction of a cooling sun, but according to some of its most ardent champions was intercalated between warmer periods, and according to others of its champions was only one of a succession of ice ages, is, it seems to me, to squeeze sunbeams out of cucumbers, a process no longer admissible.

Other writers have postulated for the sun an intermittent heat, due to the intermittent character of the supplies of fuel which reach it. Thus the late Mr. Searles Wood, junior, quotes approvingly an unanswerable dictum of Croll's that our luminary cannot have been giving out heat from all eternity, and it seems clear that the heat which it radiates continually must diminish its heat capacity as a furnace, unless it be supplied with fuel to replace that energy which has been dissipated. Mr. Wood holds that the sun's heat is in fact maintained by chemical action upon material diffused through the medium in which are moving the sun and all the other members which make up the sidereal agglomeration of which the Milky Way affords a longitudinal view. He urges that it is reasonable to suppose that the material thus diffused may vary in different parts of space, and the chemical action may vary in intensity accordingly. The many instances, he says, which have of late years been observed of coruscations or changes in the brilliancy of several stars (as distinguished from the periodical changes of many, such as Algol), and the short duration of some of these changes, strengthens the chemical theory, and he quotes the names of Grove, Williams (in his paper " On the Fuel of the Sun "), C. W. Siemens and Sterry Hunt as favouring this view of the recuperation of the sun's forces (*Geol. Mag.*, 1889, p. 301).

The view, however, is based upon mere imagination. We have no warrant of any kind for supposing that the sun's passage through space brings it at times into areas where it finds plenty of fuel, and presently into other areas where there is a scarcity of such materials. The notion that streams of meteorites may tend to keep up the sun's heat is a possible conjecture, but we have no reason to suppose that such streams are other than normal elements of the solar system itself, and that they are to be found forlorn and stranded in various parts of space, through which the sun is travelling in its journey with its planets from one land of mystery to another.

I will conclude this dissection of the various theories of a glacial climate, based upon a varying solar radiation however caused, with a clear and effective argument of Sir R. Ball's. He says : " The intensity of the heat which we receive from

the sun depends partly on the intrinsic heat of the great luminary and partly on the distance by which we are separated from him. If either the distance of the earth from the sun, or the intrinsic heat of the sun, be altered even to a small extent from its normal value, climatic changes will result; and these changes may seem altogether disproportionate to the cause which has originated them. In our search for the cause of the ice age, we have therefore to examine the possible fluctuations which the sun's heat may experience, and we must also consider the fluctuations of the earth's orbit, upon which the quantity of the heat which we receive from the sun is dependent.

"Is, then, the sun a capricious source of heat? Does he really discharge for us a supply which is at some periods more copious than at others? At a first glance we might think that this may indeed be the case, for there are no doubt some mysterious periodic phenomena associated with the sun which are at present but little understood. The outbreak of sun-spots in certain years attains a maximum, and an undoubted rhythmic succession of such years of sun-spot maxima has been discovered. But it is quite evident that the phenomena here referred to are so brief in their period that they are wholly without any influence on ages so immense as those which glacial phenomena require.

" We must also remember the great astronomical truth that our sun is only a star, and we are well aware that among stars there are not a few which are properly called variables. There are stars whose brilliancy waxes and wanes with more or less regularity; some of them diminish occasionally and invisibly, while at their brightest they are eminently conspicuous objects. Certain classes of variable stars go through a systematic series of changes, and repeat those changes during thousands of cycles with undeviating uniformity. It has therefore been sometimes thought that as our sun is a star, he may also be a variable star. But there is really no substantial ground for such a surmise. It seems almost certain that what we call variable stars are only variable in their light in much the same way as the beams from a revolving lighthouse exhibit periodic changes to a mariner. The variable star is doubtless in rotation itself, and is attended

by other objects which revolve around it. The variableness which it presents to us is certainly in some way a consequence of these rotations, though it cannot be denied that a good deal of obscurity still surrounds the subject. There is not the slightest reason to believe that the alterations are due to any actual shrinkage in the volume of the radiation. What happens is that a portion of the light is sometimes intercepted or sent elsewhere; thus the variable star appears to change its lustre, or sometimes becomes altogether invisible. This analogy is really of no service in the attempt to explain the cause of the ice age. It is perfectly certain that our sun is not a variable star in the sense in which we speak of an object like Mira Ceti as a variable. It is also certain that even if the sun did possess a character of this description, the circumstance would give us no aid whatever towards the solution of the particular problems presented by the ice age.

"I do not, however, assert that from age to age, from one million of years to the next million of years, the radiation which the sun dispenses must be absolutely uniform. Indeed we know that this can hardly be the case; the sun must obey the same laws of heat which we have studied in our laboratories. Radiation involves loss, and larger though the sun is and intensely heated though it is, our luminary has still only a certain capital of heat available for all purposes. The crash of meteors will, no doubt, serve to supply heat which will compensate in a small degree for the torrent which is perennially dispersed by radiation. It cannot, however, be doubted that the sun discharges more than it receives, so that at the close of each century it contains much less heat, actual and potential, than it had at the beginning. It seems almost a paradox to say that the sun is losing heat, and then to add that it may yet not be falling in temperature. It may be, indeed, that the sun during a large part of its career actually rises in temperature, notwithstanding that it is continually losing its heat. I do not now dwell on this matter. I have only introduced it for the purpose of showing that there is no intrinsic variation in the sun's heat which would account for such phenomena as ice ages. The solution of the problem must account for the disappearance as well as for the appearance of ice ages, and for the appearance as well as

the disappearance of those glacial intervals by which succes-
sive glaciations have been divided. The changes that are
possible in the sun's radiation will not explain such a succes-
sion of contrasted phenomena. There can be no doubt that
in stupendous periods of times past the intensity of the sun's
radiation has performed a deliberate progress. Compared,
however, with the changes in the heat-emitting power of the
sun through past uncounted millions of years, the coming of
ice ages and the going of ice ages can only be described as
the merest flutter. So far as our present inquiry is concerned,
we may conduct our arguments on the supposition that the
intrinsic radiation from the sun has remained at its present
value throughout that recent chapter in geological history
which treats of the distractions of our globe by glaciation "
(*The Cause of an Ice Age,* pp. 48-52). It seems to me that this
could not be better put, and that it is conclusive.

Let us now turn to another theory which has been pro-
pounded to account for an ice age, namely, the possibility
that the earth may at some time have passed through regions
of space warmer or colder than those in which it is now
travelling. Upon this I also enlarged in my previous work
(*Glacial Nightmare,* pp. 333-336).

Poisson admitted the possibility of the earth having at
some time travelled in space so hot as to melt all its rocks
(D'Archiac, *Histoire des progrès, etc.,* i., p. 21). Renoir and Heer
similarly postulated regions of space much colder than that
in which it now finds itself. It is clear that to speak of space
itself as either hot or cold is a misuse of terms; what is, of
course, meant, is that there may be a difference in the amount
of force in the form of heat passing through space.

Mr. Upham says of this view: " To this suggestion it is
sufficient to reply that the researches of Prof. S. P. Langley,
now secretary of the Smithsonian Institution, show that at
the present time no appreciable measure of heat comes to us
in that way, and that probably not so much as one degree of
the average temperature of the earth's climate was ever,
within geological times, so received from all other sources
besides the sun and the earth's own heat. Concerning the
latter, also, it is well ascertained that during, at least, the
Mezozoic, Tertiary and Quaternary eras, it has affected the

climatic average by no more than a small fraction of a degree" ("Causes of an Ice Age," *Trans. Vict. Inst.*, pp. 5, 6). Sir R. Ball has also answered the views here contested so clearly and graphically that I am tempted to again quote from him, and to quote at considerable length. "I confess," he says, "that they seem to be fanciful notions wholly unsupported by the known facts of astronomy. The temperature of space is rather a vague expression, and I do not feel able to attach any meaning to it, except in so far as it is an indication of the heat received from the stars. If, therefore, our solar system has at any time experienced a space temperature different from that of the region through which it is now passing, it follows that there must have been at that epoch a supply of star-heat communicated to the system different from that which it now receives. Let us suppose, for instance, that the system did enter a part of space where the temperature was sensibly higher than we find it now to be. Some of the stars must then have been either intrinsically much hotter or else very much closer to the sun than they are at present. Does there seem any probability that this has been the case? At the outset of our inquiry we are unhappily rather baffled by a dearth of precise information. We have little or no knowledge of the amount of heat we at present receive from the stars, though, no doubt, indications of such heat have been recognised by instruments in the hands of skilled observers. If, however, we regard the heat which each star sends forth as being on the average proportional to the star's brightness, we can make an estimate which will suffice for our purpose. The lustre of several of the stars has been carefully measured by photometers, and it has been found possible to express the relation numerically between the light radiated from one of the stars and the light dispensed by the sun himself. It has been shown that the most brilliant star in the whole heavens, the peerless Sirius, sends to us an amount of rays which, though much in excess of those emitted by any other star, require multiplication by a formidable array of figures before they can be brought to equality with those from the sun. Sun-light transcends the light from Sirius as received by us in the ratio of 20,000,000,000 to unity. Let us suppose that the sky contains 2,000 stars,

each of which transmits to the earth as much heat as Sirius sends us (this is a liberal supposition, for we have exaggerated the total amount of star-heat, which is not at all likely to be nearly so much as 2,000 times that of the heat due to Sirius); and even with this assumption it would seem that the total amount of star-heat is not the ten-millionth part of that which we receive from the sun. I have shown that a fluctuation of sun-heat to the extent of one-tenth would be amply sufficient, so far as mere questions of temperature are concerned, either to produce an ice age or to disperse it. As, however, the star-heat is only one ten-millionth part of the sun's heat, it would seem that unless the capacity of the stars for radiating heat were 1,000,000 times as great as it is at present, it would not be a factor sufficiently potent to dispel an ice age by its presence, nor could the advent of an ice age be the consequence of its absence.

" It is therefore obvious that, with the present relations of the sidereal system to the sun, the stars give no aid to our search for the cause of the great ice age.

" It would be proper to inquire whether this relation has always been as it is at present. No doubt the twinkling stars are in these days incompetent to exercise any climatic influence. Can it, however, have been possible that a temporary increase in the splendour of particular stars might have been an appreciable source of heat ?

" Stars of exceptional lustre occasionally burst forth in the sky, and they sometimes attain no little brilliancy. Indeed it has happened that a temporary star has so far outshone all ordinary stars as to be conspicuous with the unaided eye in broad daylight. But such phenomena, however interesting, render no service to the present inquiry.

" There has never been an outbreak among the stars, so far as we can tell, sufficiently important to produce the slightest possible effect upon climate ; we cannot, therefore, look to their variableness for any aid in explaining the occurrence of the great ice age.

" There is, however, another way in which it might be contended that star-heat may have occasionally risen to be a far more important climatic element than it appears to us to be at present. Can our system, in the course of its journey

through space, have chanced to have voyaged into the vicinity of other stars ? We know that many of the stars are in actual motion. Indeed, out of all the myriad hosts that the sky contains, it seems inconceivable that even a single one should be found which lies actually at rest. It is no doubt true that many of the stars have not as yet exhibited to us an amount of motion which is appreciable by our measurements. As, however, years roll on, one after another exhibits sufficient displacement of its position to arrest the astronomer's attention, and thus the so-called fixed stars are gradually being discovered to be endowed each with its proper motion.

" Although the sun is of such unrivalled importance to the earth as the source of our light and our heat, still among the general host of heaven the sun has no more importance than is possessed by thousands of other suns of equal, and in many cases of far greater, splendour. The sun is but a star around which the planetary system is clustered; nor has the sun a fixity in the heavens which is denied to the rest of the sidereal system. As the other stars are moving, so also the sun is following a definite route through space, and is accompanied in its journey by the earth and the other planets which own its sway.

" When we attempt to express the velocities of star-travel by the ordinary standard of miles per hour, no doubt they seem enormously great. These movements are not, however, to be described as rapid when we remember the size of the theatre in which such grand phenomena are developed. There can be no doubt that as the stars are moving, one and all, over the face of the sky, the sidereal heavens must be undergoing a continuous transformation. Its progress is, however, very tardy when estimated by ordinary chronology.

" The constellations seem to have existed from the remotest antiquity of which any record is preserved. The Great Bear of which Homer sang is the Great Bear of our skies to-night. The constellations are as old as the hills, no doubt, but have the hills themselves existed from all antiquity ? In the progress of geological periods of time, hills and mountains have been upheaved, and hills and mountains have vanished away.

" The noblest and loftiest of mountain ranges, the everlasting mountains as they seem to our view, are but transient

objects, the mere sport of geological forces. The fitful ocean, which ripples to every breeze, preserves its general form unaltered from age to age, while the mighty mountain, reared of solid granite, with its foundations deep in the earth, is upheaved in one age only to be dissolved in the next.

" The constellations in the sky may be permanent so far as man's observation has extended, but when adequate time is allowed for the stellar hosts to manœuvre, we see that our constellations have no more permanence than have our mountain chains. The family groups of the Great Bear and Orion, as well as the other striking collocations of stars, are undergoing ceaseless transformation. Sometimes the old forms are being relaxed, sometimes, doubtless, new and picturesque arrangements are being produced. In the course of ages those stars which adorn the heavens at the present moment must be transformed, nay, must sink into invisibility and be replaced by other stars, forming new constellations which will have no greater permanence.

" Considering the transitory nature of all the objects in the heavens, no less than of the objects on the earth, it has been thought that great climatic changes may thus find their explanation. Suppose that in the lapse of ages that star which we call the sun shall have adventured so near some other star as to have experienced an increase of temperature, may not great alterations of climate have arisen ? It is, indeed, hard to see how such an event, even if it could have happened, would give promise of throwing much light on the occurrence of an ice age, though it might conceivably be invoked to provide an explanation of a warm period. It is, however, useless to discuss the matter, for astronomers will not admit the possibility of any approximation between our sun and another star sufficiently close to account for any considerable derangement of the conditions of temperature on this globe. The astronomer remembers that a body cannot radiate heat in vast quantities unless that body also possesses considerable mass. Any body possessing mass will exercise attraction on all other objects around.

" If, therefore, the supposed body were near enough and large enough to radiate appreciable heat, it must also have

been both near enough and massive enough to effect quite an appreciable derangement in the movements of the planetary system. The several motions of the planets around the sun are adjusted in delicate relationship. We cannot, indeed, assert that external agency has never created a disturbance of the organised arrangements, but we may feel confident that the motions of the planets exhibit no trace of having suffered, even during geological times, any great interference from outside influence. We are, therefore, assured that during such periods as those to which the ice age has reference there has been no close approach of our solar system to any large stellar body. We cannot, therefore, believe that any exaltation of the earth's temperature has taken place by approach to a star. We conclude, then, that the circumstances attending the voyage of the solar system through space offer us no aid in attempting to account for the climatic revolutions of the ice ages.

" We are thus compelled to dismiss the supposition that the changes of geological climate can be attributed to variations in the supply of star-heat; still less can we account for such changes by the assumption of fluctuations in the intrinsic intensity of the heat radiated from our own star, which we call the sun. Such explanations would certainly, if they could be substantiated, offer a bold and at all events a readily intelligible solution; but there is no such heroic method of dealing with the problem " (*The Cause of an Ice Age*).

M. Babinet (*Revue des cours scientifiques*) has published a theory, according to which the earth, during the glacial period, was travelling in regions of space occupied by cosmic clouds which intercepted the solar radiation. This is again a purely arbitrary hypothesis, so far as I know based on no positive evidence, and contradicted by the fact that if such regions of space existed they would interfere with stellar radiation of light, of which we know nothing.

This virtually exhausts the various theories which have suggested themselves to the ingenuity of man to account for an ice age, and which are dependent on causes astronomical and otherwise, more or less extraneous to the earth itself, and, as we have seen, they are all helpless and impotent. We are no better off if we appeal to causes purely mundane.

Most of those that have been suggested I have already criticised in my former work.

It is hardly of any use labouring arguments which no one disputes in regard to the effect of the internal heat of the earth on climate. It is perfectly plain that whatever initial heat the earth's surface may have received from its interior when it was still a molten mass (if it was ever so), it ceased to be appreciable when it had become encased and covered by a slag of cooled materials. Lord Kelvin states the case in trenchant terms. He says "underground heat cannot have ever sensibly influenced the climate. Ten, twenty, thirty times the present rate of augmentation of temperature downwards could not raise the surface temperature of the earth and air in contact with it by more than a small fraction of a degree Fahrenheit. The earth might be a globe of white hot iron covered with a crust of rock 2,000 feet, or there might be an ice-cold temperature everywhere within fifty feet of the surface, yet the climate could not on that account be sensibly different from what it is, or the soil be sensibly more or less genial than it is for the roots of trees or smaller plants. Yet greater underground heat is the hypothesis which has been most complacently dealt with by geologists to account for the warmer climates of ancient times" ("Geological Climates," *Trans. Geol. Soc.*, Glasgow, 22nd February, 1877).

This is conclusive; but even if it were not so, it would be difficult to account for a climate subject to alternations of cold and heat, and in which a supposed glacial age was intercalated between temperate periods, by such a cause. If the similar change in the earth's climate is due to the loss of its initial heat, how can it have recovered again from the monstrous effects of an ice age?

So much for the bold and bare theory of a diminishing temperature of the earth's surface due to internal cooling. Let us now say a word or two about more fantastic notions.

M. Renoir, who had already published and renounced two other theories to account for a glacial climate, published a more extravagant notion in the Bulletin of the French Geological Society in 1840. He argued that the earth once revolved at such a distance from the sun as put it out of the reach of any enhancement of its temperature from solar radiation.

When thus situated it lost its own internal heat, and in consequence its surface became icy cold, and the water and vapour upon it became frozen. This he claims was the real ice age. The friction of the ether in space, he further argues, caused the earth to gradually approach the sun, not directly but in enormous spirals. This brought it within the influence of solar radiation, which melted the ice, and hence the more genial conditions of post-glacial times. This most fantastic theory was elaborately analysed and answered by M. Fauverge on astronomical and M. Angelot on geological grounds (*Bull. Soc. Géol. de France*, xii., pp. 94 and 308). I will only say of it, in the words of Falsan, " Il est inutile de répéter ici les objections opposées à des hypothèses qui n'étaient en définitive que de simples jeux d'esprit ".

A somewhat similar theory has been recently started in America by Dr. Marsden Mason ("Geological and Solar Climates : their causes and variations," *Department of Geology and Physics*, University of California, May, 1893). He is a civil engineer rather than a geologist, and a believer in the most extravagant developments of the glacial theory, and notably of tropical glaciation on a great scale, and he claims that there are cosmic laws whose combined action must have subjected tropical areas to glaciation as well as other portions of the globe. There is some difficulty in making out his real meaning from the mass of involved rhetoric in which it is buried. So far as I can understand his extraordinary theory, it is as follows :—

1. A hot spheroid revolving in space, holding water within the sphere of its control, must be surrounded with thick clouds of vapour, which will prevent it receiving heat from the sun or from being cooled by radiation, except from the outer surface of the closed envelope, which will be largely compensated by the heat reaching it from the outside.[1]

2. The surface temperature of such a spheroid will be independent of exterior sources of heat *until the water surrounding it shall be converted into ice or be reduced to its point of maximum density*. This extraordinary view I cannot follow. It is clear

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[1] The author's words are that " vapour *is absolutely impenetrable* to heat rays . . . no ray of heat could penetrate the cloud ".

that the blanketing effect of clouds as we know it in nature is not absolute but relative only, and not only so, as the spheroid cooled down to the point when the water was no longer largely evaporated and formed thick clouds the radiating heat of the sun would more and more penetrate the moist air and quite prevent it from sinking to the freezing point. The author seems to fancy that the clouds he summons would absolutely bottle up all the internal heat and prevent any accession of solar heat until all the water *was frozen into ice.* The logic of this writer, in fact, seems like an argument from *Alice in Wonderland.* He then argues that until the complete exhausting of all internal heat capable of radiation (there being no supply from the outside possible), all the earth's surface would have one mean temperature from pole to pole varying only with elevation or local causes, and that a series of uniform climates must have prevailed independent of latitude and decreasing in temperature until glacial conditions ensued, and that these conditions must also have been independent of latitude. That is to say, he argues that the earth while gradually cooling would go on doing so until all the water upon it was frozen, since it would be cut off from any supply of heat from the outside.

Thereupon, and when the surface heat was thus exhausted, the first rays from exterior sources would reach the planetary surface. These, he says, would be trapped and absorbed, and a gradual accession of heat would be inaugurated, resulting in the removal of glacial conditions and the inauguration of a new distribution of heat arranged in zones and subject to solar control.

The glacial period, then, according to this extraordinary theory, was the culmination of a gradual cooling of the earth caused by the exhaustion of its own heat, and it ended in the inauguration of a climate due to solar control. The ice age, we are told, only culminated when the oceans were reduced to a degree of intense cold. " Such glacial conditions may have existed locally or temporarily at any latitude provided ,there were elevated portions of the crust or temporarily favourable conditions. The earth might have been subjected to double the solar energy which has prevailed in the past, and yet glaciation would have been just as certain to occur ;

it would merely have increased the rate of the removal of glacial conditions. Similarly, a decrease in the volume of past solar energy would simply have retarded the rate of removal of glacial conditions."

It is almost incredible that a theory involving a revolt against the most elementary laws of heat radiation and of physics, and so much at issue with all the facts of geology, should even have been published. It would not have been worth while to state it at this length and in this way to let it refute itself if it had not received considerable countenance in America, and if it had not also been printed at length in the organ of the younger English glacialists (*The Glacialist's Magazine*), where it occupies seventeen pages (*op. cit.*, ii., pp. 90-100 and pp. 107-114), and where the memoir in which it was published is called *a remarkable paper* (*ibid.*, i., p. 154).

In my former work I ventured, *inter alia*, to criticise unfavourably the conclusions of Prof. Frankland, who suggested a theory to account for an ice-period dependent on possible cooling of the crust by the ocean waters (see *Phil. Mag.*, ser. iv., vol. xxvii., p. 321 : this I criticised in my *Glacial Nightmare*). Prof. Frankland pointed out to me that he afterwards withdrew his theory, a fact I did not know when I wrote my book. In his collected papers, entitled *Experimental Researches*, p. 960, he says of his former hypothesis :. "At the time it was proposed the temperature of the deep sea was entirely unknown ; but since the researches made on board H.M.S. *Porcupine* and *Challenger* the chief part of this theory has become untenable, for it is inconceivable that a process of heat convection, which had gone on down to the very recent period of European glaciation, should now have so far ceased as to allow the floor of the deep ocean everywhere to have assumed approximately the temperature of freezing water ". This withdrawal, which is worthy of a great philosopher, who, when he finds he has made a mistake, as we all must sometimes, confesses it and cancels his theory, dispenses with further criticism.

While some have appealed to changes in the thermic condition of the solid earth and the ocean, others have turned to changes in the atmosphere. Thus, Mr. Culverwell having demolished Ball and Croll, proceeds to propound a glacial

theory of his own. He suggests that the earth in its travels through space has been gradually acquiring a larger and larger modicum of air by attaching to itself the gases which it has met with in its voyage, and that consequently the atmosphere is now much greater than it once was, and that when it was much less dense the climate would be much more severe. I confess I cannot quite understand how this theory of my accomplished friend, which is a purely arbitrary and hypothetical one, can be supported by any serious facts. We do not know that there are any loose gases ready to be picked up in this way in space; if there are, they cannot, under the conditions of the temperature of space, exist otherwise than in the solid or liquid form. Nor can I see how this theory can be made to square with the fact that the climate of the earth during nearly all geological times has been hotter and not colder than now, and that the postulated glacial period was a mere cold phase in a long drama during which the earth has passed through much hotter conditions than now. Not only so, but if the earth was able to pick up vagabond gases in this way, why not the moon, which is remarkable for its very slight atmosphere, and several of the planets which have very slight atmospheres too? Was the earth especially favoured in this way by having its route attended by these strayed or primeval gases? In the cold of space they must be condensed into fogs or frozen particles, of which we have no evidence in the aberration of light, etc., passing through them. In every way this hypothesis seems quite out of sympathy with any results of empirical science.

Prof. Whitney held that the ice age was directly due to the greater humidity of that period affording larger materials for snow, etc., and that the disappearance of glaciers is due to the general drying up of the earth. I do not know where the evidence is that in the earlier geological periods there was more humidity than now; and if there had been, the farther back we travel in time the colder ought the climate to have been, since the same cause would be operating in an enhanced degree, which is contrary to all the evidence. Dr. Wright has a more concrete answer to this view. He says this theory " is ruled out by the fact that there is evidence, among other things, from the vast deposits of salt existing

in numerous parts of the world, that the work of desiccation has been going on in some portions of the earth from the earliest geological ages. For example: central New York is at the present time one of the best-watered portions of the world ; but it is underlaid by deposits of salt sixty or seventy feet in thickness, and these extend under much of the area of Upper Canada and Michigan. To produce this amount of salt there must have occurred, during the upper Silurian age, the drying up of an inland sea over that region a mile in depth. We are compelled, therefore, to regard the era of the saline group of rocks, rather than the present, as the great age of desiccation. Besides, moisture in the atmosphere is efficient as a glacial cause only when it is precipitated as snow, and this must be determined by general meteorological conditions. There is probably moisture enough always in the air to produce an ice age if the conditions can be combined to precipitate it in the right form and at the right place to encourage the growth of glaciers."

Some people, again, have supposed that the composition of the air itself may have so altered as to affect its diathermancy, and thus affected also its rate of radiating heat. The only way in which the air has so changed, so far as the facts suggest a change, is by a diminution in the amount of carbonic acid in it; but carbonic acid, like vapour, has a very large blanketing effect. Its increase in the air might cause a warmer but not a colder climate.

We have now run the gauntlet of the various theories which are known to me, and which appeal to causes of an ice age either external to the earth or purely terrestrial, which involve a general lowering or raising of the temperature of the world as a whole or of one of its hemispheres, and I may here set against them all *en bloc* a frailty which seems completely fatal. It has been urged by Lecoq and Tyndall most effectively. The former in his memoir, *Des Glaciers et des Climats, ou des causes atmosphériques en géologie* (Paris, 1847), boldly declared what then seemed a paradox—that in order to create an ice age more and not less heat was required. This view was pressed home in later years by Tyndall with his usual force. He .says, *inter alia,* " every pound of ice piled up on the land represents an amount of heat sufficient

to melt five pounds of cast-iron; and what has to be accounted for is not what superficially would seem probable, namely, the coldness of the ice, but the vast quantity of heat stored up in that same ice in any glacial period.

" It is perfectly manifest that by weakening the sun's action, either through a defect of emission or by the steeping of the entire solar system in space of a lower temperature, we should be cutting off the glaciers at their source. Vast masses of mountain ice indicate infallibly the existence of commensurate masses of atmospheric vapour and a proportionately vast action on the part of the sun. In a distilling apparatus if you require to augment the quantity distilled, you would not surely attempt to obtain the low temperature necessary to condensation by taking the fire from under your boiler; but this, if I understand them aright, is what has been done by those philosophers who have sought to produce the ancient glaciers by diminishing the sun's heat. It is quite manifest that the thing most needed to produce the glaciers is an *improved condenser*. We cannot afford to lose an iota of solar action. We need, if anything, more vapour, but we need a condenser so powerful that this vapour, instead of falling in liquid showers to the earth, shall be so far reduced in temperature as to descend in snow " (*Heat as a Mode of Motion*, p. 189).

" It follows from this," as Mr. McGee says, " that profuse evaporation and complete condensation cannot take place over the same region at the same time. And it equally follows that while the regions which furnish and those which condense vapour may be contiguous, they must be quite distinct."

If we are to distil rain and snow, we must have a hot boiler in one place and a cold condenser in another, and both acting at the same season. As we have seen that most of the moisture in the air which rises between the tropics falls again in the shape of rain in the same district, it follows that whatever we do to cool the arctic regions will give us no more snow there unless we increase the vapour in the districts whence the ice must be fed.

This conclusion has been also neatly expressed by an anonymous writer, who says : " It must be remembered that

a glacial epoch is not merely a season of unusual cold. Its characteristic feature is the 'glaciation' of a considerable portion of the land; in other words, its becoming covered with a continuous mass of glacial ice. This phenomenon cannot obviously arise from any general decrease of the earth's temperature. Were the sun to be extinguished, evaporation from the surface of the globe would cease, and all downfall, either of rain or snow, would cease likewise. Though, therefore, in such an assumed case the ocean would be frozen down to its very bed, the land would remain bare of ice. For glaciation it is necessary that evaporation should go on unhindered in some warm parts of the globe, and that the moisture thus volatilised should be transferred to colder regions and there precipitated. This consideration at once disposes of a variety of theories concerning the probable cause of a glacial period " (*Quarterly Journal of Science*, xii., p. 310).

It disposes in fact of all causes whose action is not differential, but which act upon the earth or its climate as a whole in the same direction. We must now turn to the only theories of a glacial age still left for discussion: namely, those which attribute it to changes in the contour of the earth's surface, and therefore to more or less local causes. These theories are of two kinds.

The American geologists have largely adopted the view that the ice period was caused by former *elevations* in the earth's crust, a view which they call the epeirogenic theory of the cause of an ice age. This theory was apparently first promulgated by Prof. James Dana in his presidential address to the American Association in 1855, and the term epeirogenic was first applied in this sense by Mr. G. K. Gilbert in his *United States Geological Survey* (Monograph on Lake Bonneville, 1890), and it has been continuously urged by Upham and other American writers. Dana argued that the fiords which exist so much in northern latitudes were valleys eroded by streams during a formerly greater elevation of the land in high latitudes. The culmination of this uplift he argued gave rise to a high plateau climate, with abundant snowfall forming an ice sheet. This movement of elevation, Dana further argued, was followed by one of depression, during which the ice sheet was melted away; and this again was

followed by another elevation, bringing the land to its present height. "The coincidence of these great earth movements with glaciation," says Mr. Warren Upham, "naturally leads to the conviction that they were the direct and sufficient cause of the ice sheets and of their disappearance."

It is clear, of course, that the raising of any considerable area above the snow line which is now below it in high latitudes must necessarily lower the general temperature greatly, and in this way greatly modify climate; but it is most difficult, if not impossible, to realise how this could be brought about so as to cause the particular glacial phenomenon which we have to explain. That there was in the so-called glacial age a much larger mass of high land than there is now near the pole itself, or within the arctic circle, or in North America or Scandinavia, which would form a great nursery of ice, is contradicted by very strong evidence, to which I shall direct attention presently. It would seem plain, in fact, that Greenland, Scandinavia and North America were all at a much lower and not at a much higher level in so-called glacial times than they are now. If this be so, then the epeirogenic theory has no base to stand upon.

In regard to this theory, another thing that strikes one is the purely arbitrary and hypothetical cause alleged for the making of the fiords. That these fiords are valleys of erosion is a stupendous postulate, to which some of us altogether refuse our adhesion. I myself hold on the contrary with that great master of geological physics, who is a great deal too much neglected in these days, Hopkins, that they are transverse fissures necessarily caused by the upheaval of the long mountain chains in which they occur, and, being so, they afford no evidence whatever of the land having been higher than now before and when they were being made. On this subject I shall have more to say in a future chapter.

In regard to the general question of an epeirogenic movement of the land, apart from the fragile witness of the fiords, the geological evidence, as we shall see presently, is against any such conclusion.

Mr. Upham himself points out two objections of another kind to his pet theory. One is that the presence of fragments of marine shells in the till both of America and of

Europe close to the sea-level precludes the idea of the sea-level having been very different to what it is at present. Mr. Upham speaks of the time when these shells were deposited *as very shortly preceding,* the glaciation. Inasmuch as they occur in the till itself, and are considered to be contemporary with it, this argument seems most inconsequent. He also speaks of the pre-glacial elevation of New England and Great Britain as having been geologically very short, while he argues that that of Scandinavia, of the northern coasts of America and of Greenland was very long. He calls this distinction, which is a pure phantasm, in so far as evidence is concerned, a beautiful illustration of the natural condition of equilibrium of the earth's crust, which Dutton named isostacy. I call it a beautiful specimen of the logical somersaults which glacialists in an *impasse* are prepared to make.

Mr. Upham seems to me to be still more inconsequent in replying to the second objection made against his view, namely, to use his own words, " the fully proved low attitude of the glaciated lands when the ice sheets attained their maximum extent and during the diversified and fluctuating history of their recession ". To this he replies that the drift deposits do not represent the period of greatest glaciation when the erosion and destruction was at its height, but only the later and closing phases of the ice age while the land was low or near its present level. The comparatively much longer early phase of high altitude, the period of the ice sheets, is not it seems, *mirabile dictu,* represented by the drift and numerous moraines of the glacier retreat, or of the extreme limit of glaciation. That is to say, the glacial period only left traces of its gigantic denuding power when it had really passed away, and when the conditions under which ice sheets existed were no longer present. Surely all this is incomprehensible, except as evidence of the desperate straits to which the glacialist geologists have been forced. I shall have much more to say of this theory presently, meanwhile I now propose to say a few words upon Lyell's view.

When Sir Charles Lyell had been won over by the specious and ingenious arguments of Agassiz to the glacial cause, a cause which was sustained by arguments dear to his uniformitarian heart, he set about to discover and propound

some theory which should afford an adequate basis for the
new hypothesis. With his usual insight, he rejected the
astronomical and other causes which were so readily ac-
cepted by his less experienced followers, and fell back upon
more commonplace, prosaic, and every-day theories. He
argued, in fact, that the glacial phenomena might have been
and probably were produced by changes in the distribution
of land and water, and by this alone.

That ocean currents and winds are the great heat carriers
of the world, and the great modifiers of its climate, is plain
enough, and we have pressed the view on many occasions.
It is natural to suppose, and indeed it is pretty certain, that
similar agencies have contributed to modifying climate in
former ages. Sir Charles Lyell was well within his inductive
method in suggesting that a change in the distribution of land
and water, either in the tropics or in the arctic regions, would
have and has caused considerable changes of climate. But it is
a very long jump, and one very characteristic of Lyellian logic,
to suppose that because we can thus account for a certain
limited change on a small scale we are justified in making
an appeal to a very big one—to suppose that by any process
of manipulating the land and water surfaces of the earth
we could induce anything like the glacial nightmare of some
geologists. The diversion of the Gulf Stream would assimi-
late the climate of Western Europe to that of Eastern America,
and nothing more. In order to sensibly modify the climate
of higher latitudes in the direction of making it colder, we
must accumulate as much land there as possible and curtail
the corresponding areas in the same latitudes covered by deep
water. But the higher latitudes are already very largely
occupied by land, and there is no room for material expansion
in this direction ; and we have, in fact, evidence that during
the so-called glacial period a larger area both in North
America and in the Old World was occupied by water than
is so occupied now, and, consequently, whatever changes have
taken place in these latitudes have been in a direction opposed
to that required by Lyell's theory, and it is generally confessed
by geologists in search of a cause for an ice age that Lyell's
suggestion is entirely inadequate, even if it be permissible to
use it.

M. de Lapparent has recently changed his opinion in regard to the origin of an ice age, and, abandoning the view of M. Blandet, he now rejects all hypotheses of a cosmic nature, and appeals to geographical conditions alone, thus falling back on Lyell's view. He contrasts perpetually snow-covered Greenland with the position of Spitzbergen, which is often free from snow ; and among the geographical changes he suggests is the creation of the Mediterranean Sea in a tract which was continuous land in miocene times, which created a powerful evaporation near the great condenser, the Alps ; and he similarly invokes the replacement of the old land bridge between Europe and America by the Atlantic, which he considers took place about the same time ; and, thirdly, the divergence of the Gulf Stream, by the closing up of the barrier of Panama and the union of Florida with the Antilles.

In a similar way he postulates that the formation of the South Atlantic by a breach in a former continent uniting Africa and Brazil may have been the cause of the former glaciers which M. Sleimnann has pointed out once existed in Bolivia and other parts of the Cordillera, and originated the vast deposits of loam we find in Argentina and not in Brazil ("*Les causes de l'ancienne extension des Glaciers,*" *Rev. des Quest. Scient.*, October, 1893).

Assuredly the Belgian geologist showed the despair with which he viewed any rational explanation of an ice age when he appealed to a miocene atlantis and to the " secondary " continent, which possibly once existed in the South Atlantic, to explain the pleistocene monster whom the Rip Van Winkles of modern science have raised and cannot exorcise.

The inadequacy of mere geographical changes to explain an ice age is urged by one of the most judicial of living geologists, whose prejudices would be the other way. Thus he says : " So far as I can form an opinion, it seems to me certain that geographical changes would be sufficient to produce very marked alterations in climate, but very doubtful whether any such changes which could be reasonably assumed at so late a period would be sufficient to account for the existence of a very low temperature in so large a portion of the northern hemisphere during the colder part

of the glacial period " (Bonney, *The Story of our Planet*, p. 556).

Assuredly, also, Croll and Prof. Geikie and others have been more than justified in arguing that while geographical changes may temper climate or make it more rigorous, they are quite impotent to perform the mighty work of initiating or maintaining an ice age. I have now exhausted this part of my subject, and the analysis I have offered clearly points to the absolute impotence of every cause hitherto suggested for an ice age. I will quote some other opinions in support of this view.

Prof. Bonney says : " My view is shared by some of the greatest of living geologists. Thus it follows, from what has been said above, that the low temperature which undoubtedly prevailed during the glacial epoch has not yet received any satisfactory explanation. Each one that has been proposed is either inadequate or attended by grave difficulties. It is therefore probable that some factor, which is essential for the complete solution of the problem, is as yet undiscovered ; or, at any rate, the importance of one which is already known has not been duly recognised " (*Ice Work, Past and Present*, p. 260).

Again he says : " The question of the date of the glacial period seems, in the present state of our knowledge, to be hardly more capable of solution than that of its cause. It may be rather humiliating to make the confession, but in these problems, as in so many others, we must be content to give a hesitating answer, to state the facts and indicate the directions in which they tend, and to leave the complete solution—if, indeed, that be ever accomplished—to a future generation, which will have added to our knowledge and learnt from our mistakes " (*The Story of our Planet*, p. 561).

M. de Lapparent, writing in 1893, and speaking of the various efforts to unriddle the glacial sphynx, speaks " de l'impuissance notoire de toutes les explications jusqu'ici proposées " (" *Les causes de l'ancienne extension des glaciers,*" *Rev. des Quest. Scient.*, 1893, p. 34).

Dr. G. F. Wright says : " If this discussion of the cause of the glacial period seems unsatisfactory, the justification is that the present knowledge of the whole subject is in an

extremely unsatisfactory condition; and in this, as in other things, the first requisite of progress is to squarely face the extent of our ignorance upon the question. The causes with which the glacialist deals are extremely complicated, and yet they are of such a nature as to invite investigation, and to hold out the hope of increasing success in mastering the problem. There is opportunity yet for some Newton or Darwin to come into the field and discover a clue with which successfully to solve the complicated problem which has so far baffled us. To the genuine investigator it is a source of inspiration rather than of depression to have such an untrodden field before him " (*The Ice Age in North America*, p. 444).

Dr. Chamberlin, another very prominent leader and mouth-piece of extreme views on glacial matters in America, says : " If we turn to the broader speculations respecting the origin of the glacial epoch, we find our wealth little increased. We have on hand practically the same old stock of hypotheses, all badly damaged by the deluge of recent facts. The earlier theory of northern elevation has been rendered practically valueless; and the various astronomical hypotheses seem to be the worse for the increased knowledge of the distribution of the ancient ice sheet. Even the ingenious theory of Croll becomes increasingly unsatisfactory as the phenomena are developed into fuller appreciation. The more we consider the asymmetry of the ice distribution in latitude and longitude, and its disparity in elevation, the more difficult it becomes to explain the phenomena upon any astronomical basis. If we were at liberty to disregard the considerations forced upon us by physicists and astronomers, and permit ourselves simply to follow freely the apparent leadings of the phenomena, it appears at this hour as though we should be led upon an old and forbidden trail—the hypothesis of a wandering pole. It is admitted that there is a *vera causa* in elevations and depressions of the earth's crust, but it is held inadequate. It is admitted that the apparent changes of latitude shown by the determinations of European and American observatories are remarkable, but their trustworthiness is challenged. Were there no barriers against free hypotheses in this direction, glacial phenomena could apparently find adequate

explanation; but debarred—as we doubtless should consider ourselves to be at present—from this resource, our hypotheses remain inharmonious with the facts, and the riddle remains unsolved " (Address before the American Association, 1886).

Prof. Geikie himself speaks of " the primary cause of these remarkable changes as an extremely perplexing question, and says it must be confessed that a complete solution of the problem has not yet been found " (*Great Ice Age*, p. 816).

M. Martin, an extravagant champion of glacial views in France, says : " L'ancienne extension des glaciers est un fait; la découverte des causes qui l'ont produite sera l'honneur des futures générations scientifiques " (*Rev. des Deux Mondes*, lxviii., p. 223). This view may be put alongside of that of Heim's, which heads this chapter.

A recent writer in the *Edinburgh Review* says : " The ice age has proved a hard nut for speculators to crack. One explanatory contrivance after another has been tried without success. An astronomical theory, skilfully elaborated by the late Dr. Croll, was at first favourably received by nonplussed geologists. Meteorologists, however, with something of the *suave mari magno* sense of exemption from agitating perplexities, were adversely critical; and their objections have of late been reinforced by the slowly garnered facts of glacial geology. . . . Must then this tempting hypothesis be foregone ? It would appear so; nor are we able to provide a satisfactory substitute. Rational speculation is no longer authorised to stray along ' the old forbidden trail,' as Prof. Chamberlin calls it, of a wandering pole; and the expedient would be inadequate, even if it were admissible. The diversion of the Gulf Stream might be practicable, but would not account for the simultaneous glaciation of two continents ; and any admissible changes in the distribution of land and water are mere toys compared with the magnitude of the phenomenon that has to be dealt with. A temporarily cooled sun combines the disadvantages of being unwarrantable and unavailing ; heavy snowfall implying rapid evaporation, hence powerful sun-heat. . . . The eccentricity theory can only be regarded as out of court. It possesses considerable merits, but seems to lack the supreme recommendation of truth. And there is, unfortunately, nothing to fall back upon. All

other attempted explanations of ' the great glacial accident of prehistoric time ' strike an unprejudiced mind as hopelessly inadequate as regards the meteorology of the past. We are accordingly left, in Prof. Chamberlin's words, ' with the old stock of hypotheses on hand, but all badly damaged by the deluge of recent facts'. The riddle remains to be read " (*Edin. Rev.*, vol. clxxv., pp. 320-324). These wise words from many zealous students of geology might be increased, but they will suffice. They are most suggestive.

In this and the previous chapters I have tried to examine as fairly and frankly as I could the various theories which the ingenuity and wit of man have so far suggested to explain the stupendous mystery of the cause of an ice age. Every one of them turns out to be absolutely impotent and powerless, and as the days go by, and as the despair of the ice-men gets more and more profound, wilder and wilder do the suggested theories become. What is the rational conclusion from all this ? It is of course quite fair to say, and has been said often in the history of science, that we are bound to believe many effects for which we can assign no rational cause. That is true. But in the present case, it must be remembered, we are not dealing with facts, plain, clear, and palpable, but with an hypothesis which is disputed by some, and which is not the only hypothesis available. In such a case, surely it becomes a factor of the first moment that the hypothesis we are called upon to believe is one which cannot be equated with any of the known effects of the laws that govern the universe. Turn in whatever direction we will, we are met by the scornful silence of the sphynx. This is surely a most important fact. It must make the most orthodox of geologists heave a long sceptical sigh, and it ought to make him turn to the facts of his own science with courage and candour, and see if, as a matter of fact, they demand from him such a sacrifice of reason as is involved in a scientific hypothesis completely and absolutely transcendental and outside the realms of induction. Thither we will turn in our next chapter.

CHAPTER IV.

THE ANSWER OF GEOLOGY TO THE ASTRONOMICAL THEORY OF AN ICE AGE.

" When it becomes necessary to invent imaginary conditions, to do imaginary work instead of rigorously reasoning out the probabilities of geological facts—all too few in many cases—I shall leave the seemingly congenial occupation to the poets and romancers of science, and confess myself entirely unfitted for the prosecution of scientific investigation" (T. M. Reade, "Glacial Geology, old and new" (*Geol. Mag.*, ix., p. 87).

THE story I have had to tell in the previous three chapters is neither inspiriting nor pleasant. It is not encouraging to read of a succession of failures by men of parts and ingenuity in futile efforts to solve what is apparently an insoluble problem; to measure the waste of thought and time and oil involved in these efforts of the geological Sisyphus to roll the glacial snowball on to some stable foothold, and to see it roll down the hill in every case into the abyss where so many scientific hopes and efforts lie buried.

The prophets of uniformity are the people who chiefly deserve our sympathy. It is their purpose to find *at all times* an explanation of things not in the abnormal, the accidental, or the catastrophic; but in the ordinary commonplace machinery of nature, and it is certainly hard upon those who are wedded both to "the glacial nightmare" and to the doctrine of uniformity, that there should be such a complete and absolute failure to find any rational explanation whatever of the former in any of the known and, so far as we can see, in any of the possible operations of nature consistent with the latter hypothesis.

It is always exhilarating to watch heroic efforts and persistent courage in the quest of some difficult ideal, and it is

saddening to find one skilful and ingenious enquirer after another utterly baffled. It is more saddening to find, as we have found in the preceding pages, that, in view of the failure of every effort to base an hypothesis on empirical and inductive foundations, men have had to turn to speculations and guesses more suited to Saturnine than to mundane science, and as void of substance as a dream.

The position is a simple one. It has been supposed by some astronomers and physicists that the decision of geology involves certain definite conclusions in regard to an ice age, for which it is their duty to find an explanation. In the words of one of the most daring of the former: "Geology has appealed to astronomy to aid her in the solution of a great problem, and we are now to discuss both the question which the geologists have put and the answer which the astronomers have given" (Ball, *The Cause of an Ice Age*, p. 14). As a matter of fact, quite nine-tenths of those engaged in the study of geology in the field have utterly failed to find in the facts of their science a justification for an appeal to any such cause. While the large majority of geologists believe in an ice age, there are not many who now believe that any explanation of such a phenomenon that can command their assent is forthcoming. Of those who do so believe, there are really only two schools; the rest are individual and solitary prophets, each of his own peculiar faith. I have said enough in my former work and in the previous chapter about these eremitical philosophers and their creeds, and will now limit myself to confronting the two views which have a respectable following with the actual facts of geology. The first of these is held by the shrinking champions of an astronomical theory of an ice age in one of its various forms. The facts and views here presented in answer to them must be accepted as supplementary only to those urged in my former work in which I have traversed the same field, and I propose to show that the facts of geology instead of supporting an astronomical theory of an ice age, and offering an "explanation adequate to the effects which have been observed," are utterly at variance with the conditions it imposes.

The astronomical theory was first seriously propounded by Adhémar (*Révolutions de la Mer*). It was afterwards urged by

Le Hon (*Périodicité des Grands Déluges*), but was especially, as we have seen, pressed upon us with power and force by Croll, and more lately by Ball.

Croll was a very logical and frank person. He did not disguise the geological conditions imposed by every astronomical theory. He saw that these conditions were necessities of the problem. Hence, having adopted this theory largely on *a priori* grounds, he eagerly turned to the wide field of geology to find some support for it, and thus reversed the process as stated by Ball and believed by many others. The *onus probandi* in the first instance he saw ought to be put on the geologist, and the duty of the astronomer and physicist in this matter was to help the latter, and it was not the duty of the geologist to find some comfort for the baffled astronomer. Let this pass however, and let us turn to the several conditions which an astronomical theory of an ice age makes it imperative for the geologist to comply with.

First, as Dr. Ball says: "It is the essence of the astronomical theory that a glacial epoch in one hemisphere shall be accompanied by a genial epoch in the other, and that after certain thousands of years the climatic conditions of the two hemispheres shall become interchanged: that the ice shall leave the hemisphere desolated, and fly to the other, while the regions it has abandoned shall become clothed with luxuriant vegetation characteristic of a genial epoch. Such fluctuations seem to have occurred again and again, *in fact that they do so is a necessary consequence of the astronomical theory of the Ice Age,* . . . , as to whether these ice ages were consecutive or as to whether they were concurrent, this is not a mere matter of ordinary significance, as it involves an absolutely vital point in the astronomical theory of the Ice Age. So much is this the case, that if it could be shown that the ice ages in the two hemispheres were concurrent, the astronomical doctrine would have to be forthwith abandoned. . . . That the ice ages in the opposite hemisphere were not concurrent but were consecutive, we may feel convinced on astronomical grounds alone " (Ball, *op. cit.*, pp. 138-141).

This is quite reasonable and sound, and yet it is a little strange that among the small, if active, body of geologists who have accepted the astronomical theory of an ice age in

one or other of its forms, Prof. Penck, Dr. Wallace and Mr. Belt all claim that the glaciation in the two hemispheres was concurrent and not alternate, and apparently claim in doing so the support of the facts of geology. Can anything be more curious and more suggestive?

The fact is that the geological sphynx is not very explicit on this particular subject. So far, however, as we can either discover or interpret its utterances, they are entirely opposed to the conclusions of both parties to this quarrel. Not only have we no satisfactory evidence of alternation, but the evidence for any glacial period at all in the southern hemisphere, in the sense in which the term is used to describe the phenomena of the drift in the northern hemisphere, seems entirely wanting.

First, in regard to the postulated alternation of climates.— So far as we can see, the opinion of those who claim that parallel conditions have prevailed in both hemispheres at the same time in the recent geological age is entirely supported by the evidence. The poles are both covered and swathed with ice and snow at this moment. The glaciers of New Zealand are shrinking and have shrunk from much larger glaciers in the times that are last gone by, there being a complete continuity between them, just as the glaciers of Switzerland have shrunk and are shrinking from the size they had attained in the geological age immediately preceding our own.

Wallace says that " the traces of ancient glaciers in New Zealand point to a period so recent that it must almost certainly have been contemporaneous with the glacial epoch of the northern hemisphere ".

Neumayr, who approached the problem from the biological side, urges that it is now the universal view that the so-called glacial age was contemporary in both hemispheres.

Agassiz writes : " I have not noticed anything to confirm the idea that the glaciers of the northern hemisphere have alternated with those of the southern hemisphere in their greatest extension, as is assumed by those who connect with the precession of the equinoxes the difference of temperature required for the change. The abrasions of the rocks seem to me neither more nor less fresh in one hemisphere than

in the other ; nor do the veins of molten rocks stand out in a more or less bold relief in either case. However astronomical causes may have been connected with the climatic conditions of the world, I see no reason for believing, from any facts I have observed, that alternations of temperature in the northern and southern hemispheres have ever been the primary and efficient causes of glacial phenomena" (*Nature,* vi., p. 272).

The opinions of the local geologists in the southern hemisphere are almost unanimous, not only that it is probable the great glaciers now existing in New Zealand have a perfectly continuous history, going back to pliocene times, just as the great Alpine glaciers probably have, but that neither the débris of the fauna nor the loose deposits furnish traces of an intervening stage marked by another climate ; and be it noted the intervening climate must not only have been temperate, but, according to the conditions, a great deal hotter than the present climate, if not, in fact, in the same proportion as the postulated glacial climate was cooler. Nor are there any facts apparently available to show that the development of the southern glaciers was other than contemporaneous with the development of the northern ones, and neither preceded nor followed it. Sir Robert Ball, who claims alternation of climates as a paramount condition of his argument, is bound to confess that " the geological facts that can be appealed to on such a question are of a somewhat meagre character" (*op. cit.,* p. 30).

Again he says : " It could hardly be expected that the facts of that science (geology) could throw much direct light on chronological matters as to the order of succession of ice ages in one hemisphere and the other. We might, no doubt, find—indeed, we actually do find—traces of an ice age in the southern hemisphere just as we find them in the northern ; but though the rocks have been engraved with unmistakable characters by glaciers and ice sheets, *yet these inscriptions do not tell quite so much as we would like to know.* They assure us, no doubt, that ice ages have occurred in both hemispheres, *but they leave us uninformed as to whether they were consecutive or as to whether they were concurrent* " (*ibid.,* p. 141).

When we turn from this rather hopeless sentence to see

what Sir R. Ball has to offer us in the shape of evidence, we are indeed in the presence of a wonderful argument. He mentions the fact, attested by Sir J. Hooker, that the flora of Patagonia has some features in common with that of temperate North America. He then disclaims any sympathy with the view that these plants were created separately in the two localities just mentioned, and proceeds to explain how they came to be where they are. He suggests that the Patagonian plants in question have wandered from the northern hemisphere, having traversed in doing so the equatorial region when the conditions there were temperate. His words are so noteworthy that they should be quoted as they stand. "Darwin," he says, "mentions a group of facts which seems to make it certain that a mild, temperate climate must have occasionally encircled the earth at the equator, for how otherwise could we account for the circumstance that the plants in the high lands across equatorial Africa, India, Ceylon, the Malay Archipelago, and in some degree across tropical South America, possess common features resembling those of a temperate flora, consisting of plants that could not live on the intervening low lands? *Here we have a demonstration of that critical doctrine in the astronomical theory of the Ice Age which asserts that the equator must have been occasionally visited by a temperate climate.*"

This is assuredly a postulate and a conclusion to take away our breath. If the tropics had a temperate climate, not in times long gone by, but, geologically speaking, yesterday, what became of the monkeys and the parrots, the humming-birds and the sun-birds, and the myriads of brilliant insects which now sparkle in the sunshine of Brazil and India, of the orchids and the tender tropical plants which are such excellent indications of climate? Surely this needs an explanation. But apart from this, how is the equatorial belt to have its climate thus accommodated to the supposed necessities of temperate life by any astronomical cause? As Mr. Bulman has pointed out (*Geol. Mag.*, 1892, p. 267), the heat of the tropics would, according to Ball's own figures, be much hotter in a short summer of 166 days than in a long summer of the present duration, and the average summer temperature of the tropics would not be 80° as now, but 122°.

If, therefore, we have a difficulty in understanding how the plants of temperate regions could under present conditions cross the equator, that difficulty would be immensely enhanced if we greatly raised the temperature of the equatorial belt, unless we also argue that these plants jumped over the equatorial belt in the space of a single winter.

I cannot, in fact, follow the logic of those who deduce alternating ice caps in each hemisphere from the existence of northern plants in South America, etc., and *vice versa*. It seems to me that much simpler causes are available. They might possibly have travelled along the high lands of the great Cordillera, where they might perhaps find a highway passing through partly temperate conditions, or, again, the flocks of migrating northern birds that spend their winters in South America might easily carry the seeds of such plants. We must also remember that it is not only the land life that has to be explained, but the similar puzzle in regard to the whales of the high latitudes of both the northern and southern hemisphere. I cannot therefore see how the glacialists who champion alternating climates can find any support in biological facts for their theory, and, be it remembered, the botanical argument here quoted is the only one of any kind Sir Robert Ball has produced from the world of experience to support his transcendental hypothesis.

I must here diverge into a somewhat long parenthesis.

Dr. Ball is not alone in invoking a gigantic climatic change in the tropics in connection with an ice age. The father of the glacial nightmare, Agassiz, had done so long before him. So had Mr. Belt and others, as we saw in our former work, where their theories have been stated and answered. Agassiz's views on this subject were not a deduction from any astronomical premises, but the result of a visit to Brazil, where he claimed to have found unmistakable traces of glaciation in the tropical parts of that very hot country, and his views were endorsed and accepted by Dr. Wallace, and thus acquired a certain status in this country and elsewhere, and it is necessary that I should refer to them again.

Some of my critics have accused me of retrograde arguments, while others have urged that I have in some instances merely slain the dead and have only fought with ghosts, and

this especially in the case of the extremer and more fantastic developments of the glacial theory, such as that of tropical glaciation, which I have been told are no longer held by any one. Let us see how this matter stands. Agassiz, if not the inventor, was the great disseminator of the glacial theory, and the real father of the modern ultra-glacialist school. He was in regard to glacial matters probably unrivalled in knowledge and experience. Not longer ago than 1865 he made his famous journey to Brazil, and fresh from his glacial studies in New England he claimed to find in the most tropical part of Brazil (the valley of the Amazon) undoubted evidence of a vast ice-sheet having once filled the valley and left its moraine far out in the Atlantic. To the unbeliever and the sceptic he left a legacy of one sentence, from which I must again quote : " An old hunter does not take the track of a fox for that of a wolf. I am an old hunter of glacial tracts, and I know the footprint whenever I find it ". *(Nature,* ii., p. 272). Reviewing the well-known work of Mr. Hartt in 1870, Dr. A. Wallace, foremost among authorities on the natural history of the tropics, wrote : " It can · hardly be maintained that the discoverer of glacial phenomena in our own country, and who has since lived in such a pre-eminently glaciated district as the Northern United States, is not a competent observer; and if the whole series of phenomena here alluded to have been produced without the aid of ice, we must lose all confidence in the method of reasoning from similar effects to similar causes which is the very foundation of modern geology," etc., etc.

In 1877 Dr. James Geikie published the second edition of his *Great Ice Age,* a vast repertory of facts and opinions upon glacial matters. In that work, after speaking of Darwin's discoveries in South America, he continues : " A few years ago these observations were continued by Agassiz, who came to the conclusion that vast glaciers had descended from the mountains and overspread extensive areas in the low grounds ". Here we have, so far as an ordinary reader can gather, a quotation and acceptance of Agassiz's views, nor can I find either in the volume just cited, or in the companion volume entitled *Prehistoric Europe,* published in 1881, any word of dissent or note of warning in regard to Agassiz's supposed

discoveries. On the contrary, he goes on to quote and adopt Darwin's inferences from it, namely, that this same tropical glaciation led to the migration of temperate forms into and across the tropics, and a consequent oscillation across the hottest districts of the earth of waves of life, which would again tend to produce modifications in their structure, etc., etc.

When I wrote my book on the glacial nightmare, this, so far as I knew, was the latest pronouncement of Mr. Wallace, Mr. Geikie, and Mr. Darwin on the subject. How an issue can be supposed to be dead with such champions still living I know not. I was not aware until I heard from Mr. Wallace himself that, in his work on *Darwinism*, he had altered his views, and I therefore felt bound to produce the evidence of Professor Orton, of Hartt, Ricketts, etc., that the so-called Amazonian glacier was an hypothesis without evidence to support it, and that the widespread deposits in the valley of the Amazon and its tributaries, supposed to be of glacial origin, are, as shown by their fossil contents, of tertiary age. That in fact the track of a fox had been mistaken for that of a wolf by the keenest and most experienced of all hunters of glacial phenomena. To what I said in my previous book I must now in justice to Mr. Wallace add his own more matured views on the subject.

It was in 1889 that he published his well-known work on *Darwinism* (p. 370). On the authority of Mr. J. C. Branner, who succeeded Hartt in Brazil, and spent some years in studying its geology, he there confesses that " the supposed moraines and glaciated granite rocks near Rio Janeiro and elsewhere, as well as the so-called boulder clay of the same region, are entirely explicable as the results of subaerial denudation and weathering, and there is no proof whatever of glaciation in any part of Brazil ".

In a letter published in *Nature* for 19th October, 1893, he enlarges on the subject, and gives extracts from a memoir then about to appear from the hands of Mr. Branner. In this he states that Hartt's views on the glaciation of Brazil underwent a radical change before his death in 1877, and that he had entirely abandoned the theory of the glaciation of Brazil. Mr. Branner says that the boulders supposed to be erratics are not erratics in the sense implied, though they

are not always in place. They are boulders of decomposition, either rounded or rectangular, left by the decay of granite or gneiss; sometimes they are imbedded in residuary and therefore unstratified clays, formed by the decomposition in place of the surrounding rock. The creep of the materials, and, in hilly districts, land slides, great or small, often throw the whole mass into a confusion closely resembling that so common in true boulder clays; land slides, which are very common in the hilly districts of Brazil, apart from profound striations and facetting, produce phenomena that, on a small scale, resemble glacial till in a very striking degree.

Secondly, many of the boulders have been derived by the same process of exfoliation and decomposition from the angular blocks into which the dikes of diorite, diabase and other dark coloured rocks break up. These dikes are almost invariably concealed. The colour of clays derived from their decomposition is somewhat different to that yielded by the granites, so that when creep or land slides add their confusion to the original relations of the rocks, the resemblance to true glacial boulder clays is pretty strong. Such dikes are not uncommon near Rio Janeiro; dikes of diabase traversing granite or gneiss occur in the great mountain masses near Rio, and were not visited by Agassiz or Hartt.

Thirdly, another class of supposed erratics consists of blocks of tertiary sandstone which by exposure have changed to the hardest kind of quartzite, and when the surrounding strata are removed by denudation and a few blocks of this quartzite are left, they are so unlike the rocks by which they are surrounded that, unless the observer has given a special study to the tertiary sediments, he is liable to be misled by them.

Mr. Branner further holds that the widespread coating of drift-like materials that covers considerable areas of the country, consisting of boulders, cobbles, and gravel, sometimes assorted and sometimes having clay and sand mixed with them, is due to the denudation of the tertiary beds during the last emergence of the land, aided by subsequent subaerial denudation and surface wash.

He concludes thus: " I may sum up my own views with the statement that I did not see, during eight years of travel

and geological observations that extended from the Amazon valley and the coast through the highlands of Brazil and to the head waters of the Paraguay and the Tabagos, a single phenomenon in the way of boulders, gravels, clays, soils, surfaces or topography that required to be referred to glaciation ".

Mr. Wallace, when adopting Branner's conclusion, preaches a homily in which I cordially agree, but which was a work of supererogation to a great many of us, *viz.*, that " a superficial resemblance to drift, boulder clay, and erratic blocks in a comparatively unknown country must not be held to be a proof of glaciation, and we require either striated rock surfaces or boulders, or undoubted *roches moutonnées*, or erratics which can be proved not to exist sufficiently near to have been brought by 'creep' or land slides"; and he says "all students are indebted to Prof. Branner for having relieved them of a great difficulty, a true glacial nightmare, that of having to explain the recent occurrence of glaciation on a large scale far within the tropics and on surfaces not much elevated above the sea level ".

Dr. Geikie, in the new edition of his great *Ice Age*, says : " The traces of ice action, which Agassiz and Hartt supposed they had found in Brazil, have been otherwise explained. Their *roches moutonnées* prove to be merely the weathered surfaces of exfoliating gneiss, etc., while their boulders and morainic débris are likewise the result of the weathering of rocks *in situ*. Indeed both geologists had abandoned their former views on the subject for some time before they died " (*op. cit.*, p. 723). With the withdrawal or surrender of the last champions of an ice age in Brazil, we may treat the notion as now extinct and dead, and merely as a sample of what has passed for science in the nineteenth century.

Apart from Brazil, no new materials, so far as I know, have turned up since my former work was written in regard to other sites where so-called glacial phenomena are said to have occurred in the tropics. The occurrence of the phenomena in question in the tropics I do not dispute : I only dispute that they prove a glacial age ; and they will occupy us again presently. It is possible that in some cases the phenomena in question may have been due to local glaciers. The possibility of glaciers occurring under the tropics, if the

mountains are sufficiently high, is not disputable. They have in fact been described at Kilimanjaro and at Mount Kenya, the last by my accomplished friend, Dr. Gregory. It is possible that glaciers may similarly have existed in the Soudan and in Nicaragua if the mountains there were higher. What I utterly contest is the possibility of anything in the shape of general glaciation or of a so-called glacial period in the tropics. This I hope I proved the impossibility of in my former work, and I have nothing more to say on that behalf. Tropical glaciation however is only a parenthesis. What I was really arguing against when I turned aside was the alternation of glaciation in either hemisphere, which is a necessary corollary to any astronomical theory of an ice age, and to which, as we have seen, the facts are entirely opposed.

Let us now turn to another necessary corollary, namely, to the more directly geological evidence which is forthcoming from the several land surfaces in high southern latitudes as to there having been any glacial age at all in the southern hemisphere in recent geological times.

We must always remember that the land surfaces in the southern hemisphere which we can examine are in much lower latitudes than those available for study in the northern hemisphere. As Mr. Johnstone says: "Those who produce supposed evidences of a glacial period in the southern hemisphere corresponding to that of Europe and America overlook the fact that while the lowlands of Scotland, Wales and Ireland lie between 51° and 59° north latitude, the lowlands of Australia, to which their arguments apply, are between 36° and 38° in a region corresponding to North Africa and the middle of the Mediterranean Sea, and he says emphatically that no part of Australasia, except perhaps Stewart Island, lying at the southern extremity of New Zealand, comes within that portion of the southern hemisphere which corresponds with the specially glaciated region of Northern and Central Europe and North America. The most southerly point of Australia corresponds with Lisbon in its latitude, and is 12° nearer the equator than Ireland. If we reason from the known to the unknown, therefore, we have good *a posteriori* grounds for doubting the value of evidence which locates the effects of intense glacial action during the

glacial period of Europe in any part of the lowlands of even the most southernly region of the Australian mainland at least" (*The Glacial Epoch of Australasia*, pp. 48, 49).

If this is the case in Australasia, *a fortiori* is it in South Africa, which is so much nearer the equator? Keeping this in view, let us now turn to the localities in question, and add a few paragraphs on this issue to the detailed examination of it offered in my previous work. First, in regard to New Zealand. The reporter of the research committee on traces of glacial action in New Zealand, Captain Hutton, while detailing many marks of the New Zealand glaciers having once been greater than they are now, says that no true erratics, that is, blocks which have been transported from one drainage system to another by ice, have been recognised there. The New Zealand erratics are merely large angular boulders brought down the valleys from the sides of which they have been detached, but sometimes they have crossed from one side of the valleys to the other. No sea-borne erratics have been noticed. No true till or boulder clay containing rounded boulders, scratched or planed, have occurred there, and Hutton concludes that the ice age in New Zealand consisted of a great extension of the valley glaciers of the South Island, and that there is no evidence of the existence of an ice-sheet or of any floe-ice or icebergs in the New Zealand seas. Nor is there any proof that any of the glaciers, even at the period of their greatest extension, reached into the sea (*op. cit.*, p. 6).

Again Captain Hutton says : " The islands in the sound are not *moutonnées*, and, although some of the smaller ones are rounded, they show no signs of lee and strike sides. The precipices on either side of the sounds are also in general quite rough. I noticed only two localities where there was any appearance of polishing. . . . I saw neither grooves nor striæ," and he contrasts this with the glaciated districts in Scotland, Wales or Ireland, where nearly every rock tells the same tale. Johnstone, commenting on this and other evidence, says : " There is apparently nowhere in New Zealand any evidence of such intense glaciation as that which spread over the low levels of Scotland, Ireland and Wales, as we have no mention of anything corresponding to the 'till' of the great

northern ice-sheet. The absence of such evidence in a region whose mountains rise to a height of over 12,000 feet, and whose southern border extends to 47·10° south latitude, is full of significance when we come to consider the various theories advanced to account for the occurrence of the great glacial epoch in the northern hemisphere" (see Johnstone, *The Glacial Epoch of Australasia*, pp. 9, 10).

Captain Hutton further says : "In Europe and North America the geological evidence of a former ice age is accompanied by the biological evidence of a southernly migration of arctic shells, which subsequently became extinct as the ice age passed away. In New Zealand we find nothing of this kind of evidence, for the molluscs of the pliocene and pleistocene beds show no sign of a refrigeration in climate." Indeed several of our living shells, which are not now found in the seas of the southern parts of New Zealand, occur in miocene beds ; consequently it would seem probable that the climate of the northern parts of New Zealand has never since the miocene period been as cold as that of the southern part at the present day. Indeed a large part of the present sub-tropical fauna and flora of New Zealand was introduced from the north before the miocene period, and has flourished ever since, and this would not have been possible if there had been a great and general reduction in temperature in the pleistocene period.

Again, the islands lying south of New Zealand contain a large number of endemic species of plants and some animals ; for example, a parroquet (*Cyanoramphus unicolor*) on Antipodes Island, a duck (*Heronetta Aucklandica*) on Auckland Island, and a rail (*Rallus Macquariensis*) on Macquarie Island. These facts prove that the islands have been disconnected from New Zealand for a very long period, and during that time they could not have been covered by ice.

Lastly, we have the local occurrence of some of the warmth-loving plants and animals of the north island in isolated places in the south island, such as the New Zealand palm (*Areca rapida*) at Akaroa, and several north island shells in Stewart Island, which is hardly compatible with the occurrence of a former cold epoch, but points to a gradually cooling climate. The biological evidence is therefore to the

effect that the ocean round New Zealand has not been much
colder than at present since this miocene period (*Report Re-
search Committee*, pp. 11, 12).

While we are dealing with New Zealand, I should like to
point out a fresh argument against the astronomical theory
as taught by Croll, which has been suggested by an anony-
mous writer in the *Edinburgh Review*, already quoted. Croll
argues, as we have seen, that the conditions suited to glacia-
tion are a high eccentricity with winter in aphelion; winter
in aphelion being a potent factor. But winter in aphelion
now prevails in the southern hemisphere, and has done so for
a long time. How then comes it that the glaciers of New
Zealand, instead of being larger than formerly, " have greatly
shrunken from their dimensions at an epoch probably not
more remote than that at which the Rhône glacier invaded
the plains of Lyons? Their past extension must thus have
been independent of winter in aphelion " (*Edin. Rev.*, vol.
xvii., p. 322).

Let us now move on from New Zealand to another
southern island. In his elaborate work on the geology of
Tasmania, Mr. R. M. Johnstone says, *inter alia :* " While
admitting the evidence of former glaciation in local Alpine
regions, there is no satisfactory proof that the erratics found
in such regions belong to the period in which our raised
terraced drifts were formed; and neither in these nor in
the later deposits of the extensive lower levels do we find
any clear signs of ice action such as are exhibited so widely
in Europe and America in the shape of moraines, boulder
drift, striated blocks, perched blocks, and other huge ice-
born erratics, etc.; on the contrary, the prevailing terrace-
drifts in Tasmania are formed from materials derived from
the adjacent or underlying rocks, and with the exception of
huge boulders at the base or on the slopes of mountain
ranges, clearly traceable to gravitation. There is not the
slightest trace of rock masses which would necessitate the
agency of ice as a means of transport, if we except also those
evidences in Alpine regions in the Western Highlands, which
are, more probably, local effects due mainly to a much greater
elevation of the land in former times. The author is per-
sonally acquainted with the various evidences of glaciation

in Scotland at the higher and lower levels, and his knowledge of Tasmania is sufficiently wide to enable him to state with confidence that corresponding evidences in the latter place are entirely wanting within the tertiary and later periods" (*Systematic Account of the Geology of Tasmania*, p. 256). Again, he says, after speaking of the glaciation of Europe and North America : " There is no similar evidence of glaciation in the southern hemisphere. . . . Neither in raised sea beaches fringing the coast along Bass's Straits nor elsewhere do we find satisfactory evidence of conditions favouring the existence of animals and plants now confined to arctic and antarctic zones such as are found so commonly among the glacial drifts of England and Scotland " (*op. cit.*, p. 297 ; this was published in 1888).

In his memoir on the glacial epoch of Australasia, Mr. Johnstone says that his view in regard to the absence of evidence of glaciation on the lower levels of Tasmania is supported by the Government geologist, Mr. Montgomery. He also makes a calculation from the possible height of the Tasmanian plateau and the height of the snow-line in latitude 42° south that *there could have been no snow cap there, and hence no glaciers produced by the same general causes which produced the ice age of Europe in the pleistocene period.*

In *Nature* for 29th June, 1893, there is a letter from Mr. Graham Officer, who describes a recent visit to the central lake district of Tasmania, and he says: "Both Prof. Spencer and myself, being believers in the glacier theory of Alpine lakes, had half expected to find evidences of glaciation, especially as we had heard of well-marked signs being .found on the west coast, some fifty or sixty miles to the north-west. However, we could not find the slightest trace of glacial action. From the top of Mount Olympus, rising about 2,350 feet above the surface of the lake, we got a magnificent view of the country. . . . But in all our wanderings we did not find the slightest sign of glaciation, either in the form of moraines or of striated rock surfaces. We were not able to examine the lakes on the plateau mentioned, but from its configuration I am confident that evidences of glaciation do not exist " (*op. cit.*, p. 198).

Let us now turn to Australia. Here the whole question

we are discussing has been put into quite a fresh light by the careful and systematic researches of an experienced geologist, the Rev. J. Milne Curran, lecturer on geology in the Technical College of New South Wales. He has completely overturned the structures built up by Dr. R. von Lendenfeld, etc., upon which Dr. Geikie and other ardent glacialists have built a good deal, and which I had only means of qualifying and not of answering in my previous work. Summing up Lendenfeld's supposed discoveries, he says that that explorer came to the conclusion that glaciers extended from a high plateau (Mount Kosciusko) down into the valleys around. He noted in these valleys "most beautiful and indubitable traces of glacial action," and that evidences of glaciation were found in the shape of *roches moutonnées* scattered over an area of a hundred square miles, and added "that portion of Australia was, therefore, not so long ago certainly covered with ice". More recently Mr. Helms concludes that there are "evidences of extensive glacier action at Mount Kosciusko," and that many of the rounded, concave and level surfaces found upon a number of the large rock facings have been produced by glacier action, although the minute features of it have long since been destroyed by erosion and decomposition. In view of these positive statements, Mr. Curran made two special visits and spent several weeks in examining the district, and not merely a few days as his predecessors had done.

He begins his criticism by the remark that Dr. Lendenfeld found the strongest evidences of glaciation in the Wilkinson valley. "Now Mr. Helms admits that wherever else he saw signs of glaciation he certainly saw none in the Wilkinson valley." Just in the same way as Mr. Helms could see no evidence of glaciation where Lendenfeld found it to abound, so he himself failed to see evidences of glaciation in any of the localities indicated by Mr. Helms. Mr. Helms marks certain spots on his map blue, indicating where he found glacial traces, and others black, which he calls snowfields. "From my standpoint," says Mr. Curran, "these glacial traces have no existence, and as for the snowfields, I am able to say that on the 20th January, 1896, there was not a square yard of snow on any part of the Kosciusko

plateau. I cannot therefore agree with Mr. Helms' opinion that they never entirely disappear even in the hottest summers, and it may safely be said that they remain permanent over the limited area.

"Dr. Lendenfeld is very definite in stating that 'there was a small glacier at the head of the Crackenback valley,' and I have reason to believe he never was there. . . . Nowhere in the valley of the Crackenback or at its head could I find any traces of grooved or scratched pebbles, or any feature that would suggest *roches moutonnées*. Neither could I find any traces of moraines. Very often masses of boulders might be noted evidently transported from higher ground, but neither the boulders nor the detrital masses of which they form a part gave the least indication of glacial action. Undoubtedly, as Mr. Helms puts it, 'rocks showing rounded, concave and level surfaces' are abundant, but most certainly none of these features can without strong collateral evidence be attributed to glacial action. . . .

"Turning to the Wilkinson valley for some of the evidences Dr. Lendenfeld found so abundant, . . . I was forced to the conclusion that Dr. Lendenfeld was utterly mistaken in attributing any of the features in the Wilkinson valley to glacial action. Thus far Mr. Helms agrees with me. Dr. Lendenfeld is very definite in his statement that he found glacier polished rocks in several places. Mr. Helms could see none of these polished surfaces in the Wilkinson valley. Let me add that I could see none either."

Despairing of verifying any of Dr. Lendenfeld's observations, Mr. Curran turns more especially to those of Mr. Helms and especially to the places marked blue on his map. One of these was Lake Albina. Of this Mr. Curran gives a photograph, and adds the comment : "In the picture there is nothing to be seen suggestive of ice action. On examining the place itself, there is absolutely nothing to be found indicative of ice action. There is, in fact, no feature about the lake, the cliff or the talus at its base that may not be amply accounted for by forces actually in operation. At Lake Albina . . . no traces of glacial action were in evidence, and nothing suggestive of ice action was preserved. . . . I place it on record that in my opinion there is nothing, in the eye of the geologist,

indicative of ice action on the shores of Lake Albina." In regard to two other similar localities marked blue on Mr. Helms' map, and situated to the south-east of the lake, one at the Snowy River, Mr. Curran says, "I was forced to conclude that Mr. Helms has misinterpreted the facts observable; I could not find anything whatever of his 'glacial traces'.

"There is abundance of what Mr. Helms calls 'rock débris'. 'We could observe,' remarks Mr. Helms, 'extensive flats with large rocks sticking out of the ground out of the surface here and there, and bogs all over them,' but I am utterly unable to see what grounds there are for Mr. Helms' conclusion that 'these flats have been formed by ice'. . . . Mr. Helms, speaking of Wilkinson valley, says it may 'safely be considered attributable to glacier action'. Again he says of it : 'Entering the flat we stand on Boggy Plain, and upon an unmistakable glacier deposit'. This I consider the most astonishing statement in Mr. Helms' paper. The assertion simply bewilders one. I cannot conceive how such a conclusion could have been reached. To my mind this one fact is abundantly, unmistakably clear : Boggy Plain is not a glacier deposit. There is nothing that one can appeal to, nothing that one can point to indicative of ice action. 'Proceeding,' says Mr. Helms, 'the evidence of ice action is becoming more plentiful at every turn.' I have to state simply that I saw nothing of the sort. This was not attributable to any want of care or observation on my part. I went to the Kosciusko plateau believing that evidences of glaciation were abundant, and it was with the utmost reluctance that I was forced to come to the conclusions here recorded."

Mr. Curran, at the meeting where his paper was read, exhibited some of the boulders which occur at Boggy Plain. He says of these : "They are just of the right material, and in the proper state of preservation to show any traces of grooving or scratching—if grooved or scratched they ever had been—diorites, quartz-rock, etc. I examined hundreds of the stones, . . . but never once did I find a grooved boulder or striated pebble or a polished surface. The stones are not angular, but all are well water-worn and rounded ;

even the blocks of quartz are rounded. If these water-worn stones are the work of a glacier, I can only say that every alluvial goldfield in New South Wales is rich in 'glacial traces'—a somewhat absurd but necessary conclusion." Turning to Mr. Helms' so-called glacier valley, Mr. Curran says : " Nothing that I saw altered the opinion already expressed. Rounded rocks there are, and smoothed rocks also, with contours that probably *could* be produced by ice, but on a critical examination even that probability vanishes."

He urges that " in the so-called Evidence valley there are great piles of granite blocks broken up into rectangular masses by weathering, but no evidence of their having been got together by moving ice and none of the collateral evidences of ice in the shape of scratches, grooves and furrows on rocks, boulder clays, angular blocks, *roches moutonnées*, perched blocks, transported blocks, moraines and moraine deposits. The forces of disintegration and decomposition here are far more intense in their action than under normal conditions. We should remember, too, that we are dealing with possibly one of the oldest land surfaces on the globe.

" Dr. Lendenfeld and Mr. Helms," says our author, " have assumed throughout that there is above these supposed glaciers a gathering ground where snow could accumulate and consolidate into ice, and so form a feeding ground for the glaciers. A few hundred yards from the great glacier supposed by Mr. Helms to have come down from Mount Twynam we have the very summit of a sharp divide, with a rapid fall away on the other side. We have, in fact, a glacier without a gathering ground, a condition of things not easy to understand. Again, in regard to the glacier supposed to have filled the Wilkinson valley. In about half a mile from where Dr. Lendenfeld makes his glacier do most of its work, we come to the summit of the Divide, from which another valley dips away on the opposite side. It is reasonable to ask where were the snowfields and the gathering ground for the glacier of the Wilkinson valley ? Dr. Lendenfeld replies by assuming their existence, and from my standpoint as a geologist I protest against this assumption on his part."

Mr. Curran points out that in Dr. Lendenfeld's plate of the Wilkinson glacier he makes mountains rise tier upon tier at

the back of the range showing, as he says, polished rocks *en face.* Mr. Curran had the scene photographed, and no such mountains exist. Not only so, but standing on the highest peak of Mount Kosciusko, where the doctor sketched, "no mountains or table lands are visible above the range across the valley. . . . In a word, in regard to the supposed Wilkinson glacier and the Helms glacier, a serious difficulty exists in that these glaciers have no place to come from. It may be argued that the plateau which must be postulated in such a case has disappeared by being denuded away. Possibly, but if these great mountains and plateaux have been planed down since 'the glacial period,' there is little hope for the polished rocks of Dr. Lendenfeld or the rounded ocks of Mr. Helms being preserved.

"I found polished or rather smoothed faces on rocks at several points on the plateau. In every instance this was due to slickenside. On examining a specimen from a polished face of rock near the Garraro Tarn under the microscope, I found that the polish was due to a thin slice of silica. Now a glacier may smooth a rock and polish it, but certainly not coat it with silica. The explanation is that the point at which I collected my specimen is close to the boundary of an intrusive granite. The slate is much faulted, broken and contorted, and the specimen referred to is part of a slickenside formed at the time of the intrusion of the granite."

In regard to the similar phenomena which have been quoted from the colony of Victoria, Mr. Curran says : "As I have not been over the ground I cannot offer any criticisms from my own knowledge. It seems to me, however, that in most of the instances quoted the characters referred to glacial action could have been as well attributed to other causes. . . . The instances quoted seem to me something in the nature of collateral evidence depending entirely for its value on *the fact* of a glacial period." In a postscript to his memoir, Mr. Curran speaks of a more recent pronouncement of Messrs. Kitson and Shone in regard to evidences of glacial action in the Australian Alps of Victoria, and he says of it: "The case made out by these authors in favour of recent glacial action in the Australian Alps is no stronger than that of Lendenfeld and Mr. R. Helms".

Summing up his general case, Mr. Curran says : "I have been over the same ground as Dr. Lendenfeld and Mr. Helms. I could not but agree with Mr. Helms as to the absence of any evidence of glaciation such as Dr. Lendenfeld had reported in Wilkinson valley. But I also feel compelled to differ from Mr. Helms in respect of the other localities in which he believed he had detected evidence of 'glacier action,' and I am forced to the conclusion that the evidence is wholly insufficient, and that no striæ, groovings, polished faces (due to ice action) or *roches moutonnées*, perched blocks, moraine stuff or erratics are to be met with. Only one instance of anything like a polished block was noted, and in this case the polishing and striæ-like markings were clearly due to a 'slickenside'. Most of the granite is of a gneissic character, but normal granites are also present, the latter weathering into spheroidal masses of disintegration, the contours of which in a few cases are suggestive of ice action. There is no collateral evidence in favour of such a suggestion. Apart from local evidence, the contour of the valleys is not in the least suggestive of glaciers. I therefore concluded that (1) there is no satisfactory evidence of glaciers having once filled the present valleys; (2) there is absolutely no evidence of extensive glaciation on the Kosciusko plateau ; (3) the glacial epoch in post-tertiary times, as described by Dr. Lendenfeld, has no foundation in fact. Neither are there any snowfields with 'eternal snow,' however limited, on Mount Kosciusko" (*Proceedings of the Linnean Society*, New South Wales, xxii., pp. 796-808).

I have quoted this paper at considerable length because it pulverises a view which had gained wide acceptance, and it exposes the method by which the great glacial nightmare has been built up. I would remark that Mr. Curran's detailed researches into the phenomena he describes were made in New South Wales, where the greatest development of the Australian Alps is to be found, and of which the Victorian highlands are a mere prolongation, so that his remarks may be held to refer really to both countries. In regard to Victoria, it must be further remembered that those who have claimed to find traces of recent glacial action there have spoken with considerable hesitation. Thus Mr. G. S. Griffiths, who has

championed that view, says in a paper " On the Evidences of a Glacial Period in Victoria during Post-Miocene Times," published in 1884 : " I will preface the evidence I shall produce by admitting that the indications, if viewed separately, are ambiguous. . . . Taking rock-markings first, their occurrence here has been questioned by many. I do not claim to have seen any myself." He then refers to the researches of others, but none of them relate to Victoria, and he proceeds to the usual refuge of baffled glacialists, namely, to explain why the evidence which we ask for is not there, and to argue as if the absence of evidence in some way supports his case.

In regard to ice-scooped lake basins, which he calls " striæ on a large scale," and which we shall discuss at greater length presently, he again confesses to a great scantiness of materials, while, as we shall see, if the phenomena in question, which, as we have shown elsewhere, have nothing to do with ice-scooping, had existed, this fact would have been of no avail as evidence. In regard to *roches moutonnées* he says, " We have no example that I can hear of in Victoria ". Lastly, in regard to clays, sand drifts and gravel beds, which occur abundantly, he has to confess that they are assorted and stratified, that is to say, they are marked by the very features which exclude them from the category of true moraines.

If we turn to Gippsland, the most southern part of Victoria, and therefore the part most likely to show traces of glaciation if any had existed, Mr. Howett writes : " Nowhere in Gippsland have I been able to detect any appearance which I could in any way refer to a glacial period analogous to that of the northern hemisphere. I have nowhere met with grooved or scratched rocks, erratic boulders, moraines, or any traces of ice action, and I think that had such existed they would have been seen ere this."

Mr. Johnstone, in describing the general results of the enquiries into the supposed traces of glaciation in Australia, writes : " Setting aside for the present the origin of certain erratic boulders and other marks of glaciation which are found in beds of conglomerate in New South Wales, Victoria and Tasmania, in rocks corresponding to the close of the permo-carboniferous age, and which undoubtedly appear to have been transported to their present position by means of

floating ice, no satisfactory evidence of glacial action has yet been discovered in Australia corresponding to the till, boulder clay, *moraine-profonde* or *grund-moraine* of the great ice age of Northern Europe and North America."

Let us now turn to Africa. The question of the recent glaciation of South Africa has quite lately been discussed very profitably, in consequence of a paper read before the South African Geological Society by Prof. August Prister urging that the valleys in the vicinity of Pretoria were formed by glacial action, and that the hills surrounding them preserve most distinct traces of a recent (probably diluvial) ice period. From the sequel it will appear why I prefer, instead of giving an abstract of Prof. Prister's remarks, to quote from his critics. Mr. W. F. Hislop, while arguing in favour of South African glaciation, urged that the dolomite boulders, of which a good deal had been made, were really *in situ* over the solid bed of dolomite, there having been chemical action dissolving the carbonates of lime and magnesia in the jointings of the stone, and the spaces having afterwards been filled with earthy matter. " I have observed," he says, " in places the dolomite boulders set like huge paving stones on the solid bed of dolomite, quite detached from it, but evidently *in situ* " (*Trans. Geol. Soc.*, South Africa, iii., p. 90).

Mr. Francis points out that the hills round Pretoria are composed chiefly of quartzites and sandstones which have been exposed for countless ages to subaerial denudation, and he adds it does not seem probable that evidence of glaciation would have been preserved in rocks of such easy disintegration. Surely all traces of such an action would long since have been obliterated, especially as denudation in South Africa is much more rapid than in Europe, from the great variations between the day and night temperatures. Apparently all Prof. Prister's were on exposed surfaces and not under clay, etc. Mr. Francis further points out that in the professor's so-called moraines the *stratified* nature of the deposit was very visible. If the valleys were visited by glaciers to a depth of 200 to 400 feet, where are the great moraines that must have been formed by the erosion ? How is it, again, he asks, that granite rock and other rocks (*i.e.*, true erratics) are not lying on the strata of the Magaliesberg

formation, which formations are adjacent to the supposed glaciated area, and glaciers coming from the south must have passed over Rand sandstones and conglomerates, dolomites, granites, etc.? The boulders of greenstone arranged in lines, mentioned by Prof. Prister, Mr. Francis treats as remains of disintegrated greenstone dikes *in situ*, and the rounded surfaces of the igneous rocks as due to exfoliation. Again, according to the latter writer, the present forms of plants and animals in South Africa do not point to a recent age of ice; on the contrary, it would appear as if there had been a warm climate there from cretaceous times. " Had there been a glacial period there of such magnitude as suggested by Prof. Prister, surely we should have species of plants surviving on the mountains with arctic affinities, but so far as I am aware these are absent. . . . So far as I know," he says, " no fossil remains of an animal of a cold climate have been found in South Africa; on the contrary, the evidence points to a persistently warm climate " (*ibid.*, pp. 91-95).

According to Mr. Draper, a glacial period in South Africa would require a great elevation of the country, but we have conclusive proof in the fossils found along the coast line of South Africa that the continent has not been elevated more than a few feet since the commencement of tertiary times. Again, since the severance of Madagascar, the Mozambique current has maintained its present flow, and a glacial period is impossible in close proximity to a warm current such as that. " As we learn from fossil evidence that Madagascar was disconnected from the continent in early tertiary times, there could have been no glaciers here since then " (*ibid.*, pp. 95, 96).

Dr. G. A. F. Molengroof, state geologist, writing in October, 1897, says: " I will leave alone the question whether on general geological and climatological grounds a glaciation of South Africa in quaternary times might be considered likely or acceptable, although, by competent authorities, I personally consider that it is already clearly answered in the negative ". He then turns to the criticism of the various special instances mentioned by Prof. Prister. First, in regard to the boulders of greenstone found in the Pretoria valley as far as Daspoort he says : " These are no erratic blocks, but they are found *in situ*, indicating the outcrops of sheets of diabase, of which

they are the undecomposed remnants, and lie in clay, the result of the decomposition of the diabase.

" The chain of big boulders parallel to the main valleys on the north slope of the Daspoort range and the Meintger hill, and formed of coarse diabase, are not a lateral moraine, as Prof. Prister says, but only a line of outcrop from a big sheet of diabase laying in the Pretoria beds, under and conformable to the Daspoort quartzite.

" In regard to the angular, half-rounded, shorn, polished and tetrahedral fragments of quartzite and sandstones strewn over the ground, and the grooved, scratched, and polished rock surfaces found by Prof. Prister at the foot of and on the Magaliesberg and elsewhere, their distribution is so general that, taking their origin as being glacial, only a total glaciation of South Africa could help us to explain their occurrence. I shrink back," says Mr. Molengroof, " from this conclusion, and we can do without it. We see the same omnipotent force at work to-day which scratched and polished the rock surfaces, filled the valleys with sand and clay, and reduced hills to heaps of angular, half-rounded stone and polished fragments. It is the wind, the great denuding force of South Africa, assisted by insolation and occasionally by the transporting power of running water. All that is reported from the Kalahari, Griqualand West, and other parts of South Africa by Stow, Anderson, and others about *roches moutonnées*, polished surfaces, and other pseudo-glacial phenomena, must be attributed to wind action, as, amongst others, Schenck has already pointed out."

" Compare," he continues, " the specimens of angular fragments from Prof. Prister's moraines exhibited by him with the so-called ' facettensteine,' the results of the grinding action of blown sand described in Walther's magnificent work, *Die Denudation der Wuste* (*passim*), and one cannot fail to see the greatest resemblance. They are of the same nature, and are also like the ' dreikanter,' familiar to those who have studied the diluvium in North Germany and Holland."

Prof. Prister is much struck with the numerous transported rocks, which he argues were carried by a great glacier from the Rand mountains towards the north, filling the Pretoria valleys almost entirely. " Here I differ," says our author,

"*ab origine* from Prof. Prister. Nothing strikes a geologist from Europe so much in South Africa as the almost total absence of transport of rocks, at least in the pleistocene period, notwithstanding the enormous denudation during that period."

In regard to the Fonteinen valley, north of Pretoria, the same writer says : " I failed to find even the slightest trace of glacial action in that valley, and can only agree with the objections made by Mr. Francis against the description of the typical dolomite formation near the railway siding as a central moraine. . . . What is to be seen in this cutting is in fact the usual and typical development of the upper de-composed and undermined part of the dolomite formation. Concluding, I would give as my opinion, that a negative answer to the highly interesting question, whether we might speak of a glacial period in quaternary times in this country, has not yet been proved by Prof. Prister's statements to be the wrong one " (*ibid.*, pp. 97-100). The real meaning of this Dutch-English sentence is plain enough.

Now comes the most interesting phase of this discussion. In some notes contributed to the same journal in December, 1898, Prof. Prister says : " I fully agree with Prof. Molen-groof, and recognise that I have been mistaken in supposing that the glacial phenomena I observed in Pretoria and on the Rand were of the quaternary epoch, when they are in reality most probably Permian, as described by the doctor. I must apologise, firstly, that I am not a geologist by pro-fession ; although I have been a student of this most fascin-ating science for the last twenty years, I am sorry to say I must continue to remain an amateur. As my second excuse, it may be accepted that the traces I enumerated in my paper were so fresh that I was misled in supposing that they be-longed to a more recent period " (*ibid.*, iv., p. 151). This is a handsome confession, but it is nevertheless a good proof of the infinite mischief done to science by the publication of half-informed opinions in reputable journals in regard to matters only verifiable by those on the spot. We may there-fore take it that the ghost of a glacial period in South Africa in pleistocene times, which has troubled the geological world for a good many years, is now laid, and agree with the most

capable geologist who has examined the question, Dr. G. A. F. Molengroof, who has declared himself an opponent of a supposed quaternary ice period in South Africa (*vide op. cit.*, iv., p. 104).

This completes the evidence from the southern hemisphere, with the exception of South America. The case of South America stands entirely apart. There we undoubtedly have phenomena of the same character and on the same scale as those which have been held to justify a glacial theory in the northern hemisphere. But they form quite a local and exceptional feature in the recent geology of the southern hemisphere, and do not help in any way to fulfil the conditions demanded by Croll as a necessity of his theory. What he demands is not a local phenomenon, but a general one, in which the whole hemisphere had a part, and in which it alternated with the complementary hemisphere. Evidence of this general phenomenon is, as we have seen, nowhere forthcoming in the southern hemisphere, and thus another geological plank in the platform occupied by the champions of an astronomical theory utterly breaks down.

There is only one way, so far as I know, in which this critical difficulty of the absence or frailty of evidence has been met, and it is a convenient form of logic in the absence of all positive evidence. It has been said that we must not expect to find so many traces of a glacial age in the southern hemisphere as we do in the northern, because so much of that hemisphere is covered with water, and is not formed of dry land. But apart from the absence of adequate stratigraphical traces of the great event in such an island as Tasmania, for instance, we have the more telling and eloquent fact that among the débris of animal and vegetable life which have occurred in the recent deposits of that hemisphere there are, so far as I know, no evidences of colder conditions, or, at all events, of conditions which can be described as glacial. Upon this I quoted considerable evidence in my former work. Every one, I think, is agreed about this, and it seems to me to be conclusive.

Let us now turn to a third geological factor, which is a necessary corollary of an astronomical theory of an ice age. If the astronomical theory of an ice age be a trustworthy

conclusion, it is quite clear that, as Croll very clearly saw
and pertinaciously pressed home, the ice age in question
cannot have been a single and unique event, but there must
have been a series of ice ages at more or less regular in-
tervals coincident with the periods of maximum glaciation;
and these periods must have left abundant testimony, not
only in the remains of arctic plants and animals, but in
the more indestructible form of polished rock surfaces, far-
travelled erratics, striæ, etc., the various remains, in fact,
which loom so big when we examine the drift deposits, and
which have given rise to the glacial theory. " If," says
Croll, " the glacial epoch resulted from the causes discussed
in the foregoing chapters, then such epochs must have
frequently supervened " (*Climate and Time*, p. 266). He
presently speaks of "the secular theory demanding that
glacial epochs in past geographical periods should have been
numerous and severe " (*ibid.*, p. 290). Let us however be
a little more specific :—

Dr. Croll argued that during the last 3,000,000 years, for
which his tables were computed, there were five periods
during which the eccentricity was as great as, or greater than,
during the glacial period proper, and that, taking geological
time at the hundred of million years at which he estimates
It, there should have been some 165 such periods of cold.
" With the exception," says Prestwich, " of the Permian
which is still *pendente lite*, where is there evidence of any
such cold periods ? " (*Glacial Period, with reference to the
antiquity of man*, p. 4.) The fact is that the evidence which
has been accumulated for a long time, and notably in the last
few years, goes to show that, whatever is obscure in the
geological record, this much at least is plain, that with the
exception of the particular horizon just referred to, and to
which I shall refer again presently, namely, that of permo-
carboniferous times, there is no adequate evidence forthcoming
to support what Croll deems a necessary corollary of his or
any other form of the astronomical theory ; there is no other
horizon in the whole long vista of geological time during
which we have any phenomena comparable in extent or
character with the phenomena which have been supposed to
necessitate the postulate of an ice age in so-called pleistocene

times. The case of the permo-carboniferous beds stands apart. They no doubt do furnish somewhat of a parallel both in extent and character to the instance last mentioned. If the so-called pleistocene glaciation is to be explained by an ice age, it is plausible therefore to speak of a permo-carboniferous ice age also. The one case may be taken as complementary of the other, nay, I would go further and say that the conditions and arguments involved in the one case to some extent dominate the other. This being so, I propose to postpone the consideration of the so-called permo-carboniferous ice age until we have analysed and sifted the more recent, the more accessible and the better known phenomena of the so-called pleistocene ice age, and will at present content myself with a short survey of the evidence furnished by the other geological horizons. I say a short survey, because I have already traversed the ground in great detail in my former work, and the case as here presented against recurrent ice ages must be accepted as supplementary only to that already stated, which I think was itself conclusive.

The evidence is of two kinds : first, biological, and secondly, stratigraphical and lithological. In regard to the former, it seems to be absolutely unanimous and consistent. Go where we will, to any latitude we like, we cannot, so far as I have been able to find, trace in the remains found in the various geological beds from the bottom to the top evidence of the former existence of animals and plants adapted to glacial conditions of life, or to anything like glacial conditions, but just the reverse ; and nowhere is this more marked than in those arctic regions where, if anywhere, such evidence should have been forthcoming liberally. I have enlarged upon this at considerable length in my former volumes (*op. cit.*, pp. 455-457), to which I must refer, and it is assuredly a most eloquent and, as I think, a conclusive answer to the ardent special pleading of the few geologists who still hold on to the belief in a succession of ice ages in former times. Let us now look to the other than biological facts.

I will first say a few words on the supposed *criteria* of ice action, upon which much has been built.

In a remarkable paper read before the Cambridge Philosophical Society on the 3rd October, 1893, my old friend Prof.

McKenny Hughes points out very clearly the numerous sources of error which arise in trying to diagnose the true handiwork of ice in the older geological periods. Thus he argues that among the phenomena to be carefully sifted before we can postulate ice-action are the earth-movements occurring when the parts of a fissured rock are relatively displaced and when the sides of a fault are dragged against one another, perhaps even with a to-and-fro motion. "If the cut be clean and the rock of a somewhat homogeneous texture, it will be smoothed and polished. . . . In this way a rock as rough as, or rougher than, *the molasse* may have a soft sheen given to it. These slickensides, as they are called, often show mineral change or deposit on their faces. . . . Some minerals and rocks which are readily soluble in water, either pure or charged with a small quantity of acid, have a polish produced by the chemical decomposition of their surface."

This often happens to masses of carbonate of lime or limestone exposed to the spray from a waterfall or embedded in clay ; rocks polished in this way are generally fretted into irregular pits. Blown sand also gives a delicate polish to some surfaces, but where there are slight changes of texture in the rock the surface is also fretted and etched ; and Hughes quotes the obelisks of Egypt, the basalt of Burntisland, and the exposed silicious pebbles at St. David's. He also refers to a paper of General McMahon, who describes how certain gullies in the rocks bring the wind to a focus, as in the case of an isolated hill in the Arvali area, where deep grooves several feet in depth and diameter have been carved out of the sides and faces of huge granitic blocks by the sand-laden wind aided by the selective process of natural decay. He referred to several specimens exhibited before the Wellington Society in New Zealand, and afterwards in 1878 before the Geological Society, London, whose form was due to their lying in the path of winds compelled by the shape of the ground to carry the sand in a constant direction to and fro. The exposed part of the stone was thus chamfered off so as to leave a roof-like ridge projecting. These stones bore no striæ, but had their surfaces much fretted.

Turning from polished and smoothed surfaces to striations produced by other causes than ice, Prof. Hughes cites trees

dragged down a hillside, with bits of grit fastened to them, which have eroded the surface almost exactly in the same way that ice would do it. I would here remark that I have myself seen in many places in Switzerland hillsides where the rocks, along a long slope for several feet in width, have been scoured and scraped and fluted and grooved by the slipping of the surface soil with its contained stones.

Prof. Hughes then turns to the cases of striation and polishing due to the crushing of solid rock by earth movements under tremendous pressure acting upon surfaces in contact which are covered by irregularities, such as minerals of unequal hardness in the rock or in the fault, the harder producing flutings and scratches on the softer along the line of movement. " We may," says Prof. Hughes, "in this way find produced many of the varieties observed in the case of true glacial striæ. The rock may be fluted but not polished, or a coarse grit may' first be grooved and polished and then covered with a glistening mineral film. Sometimes a change of direction has produced secondary striation oblique to the first, and more rarely the curved lines on the mineral layer show that the direction of movement has changed. Sometimes a fluted surface is formed by the alternation of thin bands of tougher rock and softer material. When the rock mass is compressed, the latter yield and the former are forced out into ridges. Sometimes, when great pressure is applied to conglomerates, a shearing will take place throughout, with distinct thrusts and displacements between the matrix and the included pebbles, and smaller fragments of greater hardness imbedded in the parts will scratch and furrow the larger softer pebbles along which they are driven. Small tough pebbles will often deeply indent softer pebbles without breaking or being broken by them. In the case of crushed conglomerates, the pebbles are often broken and the several parts somewhat separated and shifted and re-cemented by mineral matter, and sometimes the striæ caused by slickensiding *run across the matrix and include pebbles alike.* Where the conglomerate has moved over an underlying rock, we have a polished striated and grooved floor." Prof. Hughes goes on to say: " One very striking example of this occurs in the same area as that in which so many striated blocks have been procured from the

conglomerate. It might seem that there was cumulative evidence for the glacial origin of the deposits, such as scratched stones in a boulder drift resting upon a striated surface of solid rock, but when we examine the evidence more carefully it all breaks down. This striated surface occurs along a thrust plane, which can be clearly made out on the ground; and even were it not so, the condition of the surface itself is sufficient to show that it is due to earth movement and not to glaciation. If," he adds, " there were such rock crushings as these at Holbeck Gill, it would be curious if here and there in other places we did not find that there had been a differentiating movement between masses of such unequal structure as the conglomerate and the solid grauwake on which it lay. The pebbles in such a crushed conglomerate differ from true glacial pebbles in that the grooves follow the curvature of the stone, and the edge is sometimes crushed up as if the pebble had been nipped out.

" Again, when a rock is subjected to great pressure and contains concretions or other masses of harder material, these are apt when deformation takes place to be thrust through the softer matrix, and in the process to get polished and striated. The spherical or cylindrical or conical bodies known as stylolites are thus formed."

Prof. Hughes is not the only person who has analysed some of the false and pseudo-glacial phenomena that sometimes occur. In the fourth volume of the *Bulletin of the French Geological Society*, page 55, is a paper by M. Ebray, entitled " *Striés pseudo-glaciaires* ". He recalls the fact that in the marls surmounting the diluvian conglomerates of the environs of Geneva there often occur pebbles perfectly rounded and covered with striæ, having all the appearance of glacial striæ. He says he found a Jurassic pebble which, when submitted to several glacialists, was pronounced to be a glacial pebble. One side was polished and showed numerous striæ exactly like glacial striæ. This was found on the summit of the great escarpments which dominate the right slope of the valley of the Arve to the west of the Col of the Reret near Bonneville. Having examined the pebble closely, he found that the striæ were not merely on its surface, but passed round its edges like the

marks on an ammonite, which at once made him suspect its glacial origin. He then supposed that the striæ might be due to little threads of carbonate of lime which had decayed at the surface; which view was confirmed on breaking the stone. "This," he adds, fairly enough, "is only an isolated case, but it proves that striæ may be due to different causes. On many supposed glacial pebbles it may be noticed that the striæ diminish rapidly in width and depth, and present a shape denoting a rapidly evanescent cause, and different to the continuous pressure like that of a glacier. I would ask," he says, " therefore, if they have not resulted from the torrential impact of blocks of stone against one another."

" These observations," he adds, " do not mean that I deny the former extension of glaciers, but the study of the cause of the striæ may tend to reduce the glacial theory to more rational bounds."

This is in effect the conclusion of Prof. Hughes from his analysis of the facts when he says : " There are so many ways in which stones are accidentally striated, that the greatest caution is necessary with regard to the character and origin of the scratches observed upon them; and there are so many modes of transport and imbedding of boulders, that we require the clearest evidence as to all the circumstances in which they are found " (*Recurrence of Ice Ages,* part ii., p. 233).

Murchison, speaking of the grooved and channelled surfaces of the Braid Hills, says his conviction was that these grooves, though attributed by Dr. Buckland to glacial action, are due neither to that agency nor to any rush of waters, but are simply the result of the changes which the mass of the rock underwent when it passed from its former molten or pasty condition into a solid state.

Murchison again says : " In regard to the surface of Belgium and northern France, where the boulders and glacial clay are absent, we frequently find polished and striated surfaces of the palæozoic limestones when superadjacent masses of drift have been removed from them. Such we have ourselves remarked," he says, " on the surfaces of the low hills of carboniferous limestone on the right bank of the Rhine near Dusseldorf, where, when the superincumbent

gravel is cleared away, the edges of the highly inclined beds are seen to have been truncated and smoothed down as if they had been subjected to the passage of a heavy incumbent mass, the sand at the base of which had served as a polishing powder " (*ibid.*, pp. 552, 553).

The detection of glacial effects, properly so described, requires very considerable care. Thus that excellent observer, the Rev. W. S. Symonds, says of the volcanic district of Auvergne : " It is evident no glaciers have occupied the vales since the outpouring of the later lava currents and the volcanic outbursts of the craters of the Puy de Dôme "; and he goes on to say : " Most of the country between Coyrat, near Mont Rognon, and Theix looks regularly *moutonnéed*, and may mislead any one who has not become convinced by careful examination that this appearance is owing to atmospheric weathering and the desquamation of the granitic rocks which separate at the joints and weather into rounded boulders, assuming sometimes the aspect of *blocs perchés* ". Again he says : " The position of the masses of rock called ' *les Trois Diables,*' which I believe are by some put down as *blocs perchés*, are far too close to the rocks *in situ* to allow us to attribute their transportation to a glacier rather than to a fall from the precipice. They belong to the ' *Chemins du Diable,*' which are preparing for a similar descent " (*Nature,* xiv., p. 179).

Prof. Hughes concludes that since all over the world a large number of observers have been on the lookout for evidence of this kind of ancient glaciation, the fact that the examples found are generally of very rare occurrence and doubtful character makes it necessary to remember the possible intervention of these accidental modes of producing polishing and striæ, which may be easily mistaken for those of glacial action (*op. cit.,* pp. 101-108).

Perhaps the most eloquent and remarkable fact in regard to the non-existence of glacial periods in other than very recent deposits is the marked absence of any evidence pointing in that direction where we should more especially expect to meet with it, namely, in the arctic regions. There the focus and culmination of the phenomena must always have been, and it is exactly there that the witness of explorers is most

consistent against it. Nordenskiold, who had the problem specially in view in his famous arctic journeys, and was a very skilled observer, speaks most emphatically on the subject, and I have quoted his remarks in my former work (*Glacial Nightmare*, pp. 453-55).

In summarising the results of the *Challenger* expedition, in an address to the glacialists' association, Mr. C. E. de Rance says : " It would appear that variations of climate were unknown until the beginning of the upper cretaceous period, after which a slow process of refrigeration commenced. There is no evidence of former glacial periods in the arctic area ; and it is probable that the period of cold commenced in the arctic regions during, or shortly before, the advent of cold further south " (*Glacialist's Magazine*, i., p. 9).

Let us now turn shortly to the evidence for the supposed glacial periods in former geological times which has been recently quoted by their supporters. On turning to Mr. James Geikie's treatment of this subject in the third edition of his work, I am bound to confess that it seems to me singularly disingenuous. He has remitted the facts on which he chiefly relies to an appendix. In this appendix he has published a table consisting of the following geological horizons : Precambrian, Cambrian, Silurian, Devonian and Old Red Sandstone, Carboniferous, Permian, Triassic, Jurassic, Cretaceous, Eocene, Oligocene, Miocene, Pliocene, and he adds : " The systems marked with an asterisk (*i.e.*, all the horizons here mentioned) have all yielded supposed evidences of ice action ". Then he says, " *In several cases, however, the facts may be otherwise interpreted* ". Is this the way in which a veteran geologist, addressing much younger men on a most critical issue, ought to treat his subject ?

Let us, however, dissect the evidence somewhat. Mr. Geikie himself says the Precambrian sandstones and conglomerates of Scotland occasionally assume the aspect of morainic accumulations, but *no glaciated stones have been observed.* Is not this enough ? Why not wait for such a discovery before quoting such fragile evidence ?

In regard to the same early horizon, Prof. Bonney, a very moderate and judicial glacialist, says : " In Tennessee and North Carolina certain conglomerates are found in the

Laurentian masses; in the north-west highlands of Scotland similar deposits rest upon very ancient coarse gneissoid rocks, the surfaces of which are often more or less rounded. These have been cited as indications of an ice age at the very outset of, if not earlier than, the palæozoic era, but it is generally admitted that the claim cannot be substantiated " (*Ice Work, past and present*, p. 261).

In regard to the lower Silurian conglomerates of the south of Scotland, mentioned on page 280 of my *Glacial Nightmare*, I ought to add that in the account of the deposit given in the *Memoirs of the Geological Survey of Scotland* (sheet for Ayrshire), page 7, dated 1869, it is expressly said that *no ice markings were observed upon any of the stones*, which is a significant fact admitted by Mr. James Geikie (*Great Ice Age*, p. 818).

Mr. James Geikie refers to the discovery of boulders two feet in diameter in a conglomerate of lower Silurian age at Maimanse, Lake Superior. On turning to Dawson's description, however, this conglomerate is really described as a *syenitic* bed occupying a breadth of 160 yards and forming a kind of ridge. How can this be a moraine? It is described by him as of Huronian age, and nothing is said about the kind of boulders, nor as to their presenting any glacial characteristics; and it is apparently a weathered dike.

These instances, with those criticised in my former work, complete the list of phenomena supposed to attest a glacial age in the beds undoubtedly older than the Devonian, and representing enormous deposits of strata covering a portentous period, and derived from almost all latitudes. They are, it will be seen, as fragile as they are insignificant, and whatever they point to, it is to very local causes, and nothing in the shape of an ice age. This is consistent with the universal testimony of the remains found in these old beds, which teem with organic remains, pointing unmistakably to the whole period having been marked by anything but arctic conditions. For ample evidence of this see *Glacial Nightmare*, pages 430, 431.

I ought to add that down to the end of the Silurian beds we apparently have no trace in the rocks of any subaerial beds or of subaerial conditions of life, so that glaciers and ice sheets seem on this ground alone to be out of court altogether. There may have been a frozen sea when there was no land,

but not boulder-bearing, striating, rounding ice sheets and glaciers; so that the supposed evidence of that kind which has been appealed to is out of court also.

In regard to the Old Red Sandstone deposits described by Cumming, and referred to on pages 284 and 285 of my former work, Prof. Hughes points out that the beds in question are now treated as the base of the carboniferous series, as are the similar beds from the same horizon in Cumberland, etc., described by Cumming and also by Ramsay. Hughes goes over the grounds urged by Ramsay in favour of their glacial origin: namely, (1) their occurrence in patches in the old valleys; (2) the character of the conglomerate, which is coarse and shows a very irregular accumulation; (3) the shape of the included fragments; and (4) the occurrence of scratched stones. Of these grounds he says we must remember that any subaerial or fluviatile deposit covered by an encroaching sea must have this patchy character, and adds that in its irregular accumulation the conglomerate more resembles the gravel drift of the valleys than the boulder clay, and the origin of this gravel drift is at least doubtfully glacial. In regard to the shape of the stones and the character of the striæ, although they resemble those in true drift, they have never been found except where we have other evidence that the beds have been much disturbed; and when the red conglomerate can be examined close to great faults, the beds are not crumpled up as in the Silurian or any even-bedded homogeneous rocks, but because the hard and included pebbles resist more than the soft matrix the whole mass readjusts itself to suit its new position, the included pebbles being often crunched against one another, scratched and broken. " In one place," he says, " I found inclined at a small angle to the bedding a face of jointings on which were striæ like those on the scratched stones *running across the soft matrix and included fragments alike.*" Some pebbles from this conglomerate have the less soluble portions projecting beyond the general surface of the stone, as on a piece of Bala limestone, on which the coral *Halysites catenularius* stands out in clear relief, showing all the details of its structure. In this case it is clear that the specimen has been subjected to ordinary weathering, not to glaciation. " In regard to the fossil contents Prof. Hughes

says the character of the plants and corals imbedded in the earliest deposits of the period shows that the climate was temperate or sub-tropical" (*Cambridge Phil. Soc.*, 1893, pp. 115-117).

Referring to this horizon again, Prof. Bonney writes: " There are breccias in the Devonian rocks of the Lammermuir Hills, but even Prof. J. Geikie doubts whether they can be regarded as evidence of ice action ; and thick conglomerates occur at the base of the carboniferous system in the north-west of England, the pebbles of which are sometimes striated. This, however, is now generally admitted to be the result of subsequent earth movements, and the mass itself suggests the action of water rather than of ice " (*op. cit.*, p. 262). Let us now turn to carboniferous times. Prof. Shaler was induced to attribute the thick conglomerates of carboniferous age in the Appalachian district of North America to a glacial origin. On this point Dr. Wright says : " For the most part the pebbles of this conglomerate consist of quartz or quartzite, well rounded, and seldom of larger size than can readily be transported by water, though Prof. Newberry is reported to have found a boulder of quartzite seventeen by twelve inches embedded in a seam of coal ". Dr. Wright argues that these Appalachian conglomerates are the wash brought down by large rivers heading in the mountain plateau towards the north and east, perhaps somewhat assisted by floating ice. In regard to the still lower conglomerates in Eastern Tennessee and Western North Carolina, Shaler also attributes them to glacial action, though he confesses *that no scratched boulders have been discovered in these deposits*. Here, again, Dr. Wright replies : " For all we know, the material spread out over this area of sedimentary rocks was all within reach of rivers coming down from archean heights, and so there is no necessity for supposing extensive glacial transportation from more northern watersheds ".

Let us now turn from the carboniferous conglomerates to the isolated boulders found in the coal beds. My friend Mr. Stirrup wrote a paper some years ago on these boulders, giving many details about them from which I will quote. Some rounded grey quartzite stones, varying in weight from two and three quarter pounds to six and a half pounds, were

described before the Manchester Literary and Philosophical Society in 1851 by Mr. Binney as having been found in the four-foot seam and a seam below it at Patricroft in Lancashire. They were of the same composition though found at different places, and occurred in the middle of the beds, which beds were underlaid by stigmaria roots, showing the coal had grown *in situ.* Mr. Plant in 1874 described other similar stones as having been found in the two-hundred-feet Trencherbone mine at Kearsley. They were rare, but when they occurred it was generally in groups of five, six, a dozen, or even twenty, all lying in a limited space, but separately imbedded in the coal. These boulders were, as a rule, hard silicious grits or quartzites from a pale to a dark grey colour, and clearly derived from a common source. They were *smoothed, often polished, with the corners rounded off by abrasion.* Their forms were various, roughly quadrangular, irregularly ovoid or elliptical, occasionally globular, *and all had been evidently waterworn before being deposited in the coal strata, as was shown by their rounded contours. The surface of the stones exhibited no lines or scratches, such as those seen on boulders from the glacial drift. Some had irregular furrows on them, which Stirrup attributes to the structure of the rock, or in some cases probably to pressure.*

In the Arley mine, the lowest seam of the Lancashire coal measures, white quartz pebbles are common, and they occur with others of the grey quartzite. They are mostly of small size. In the Astley pit, at Dukenfield, numerous boulders, some of very large size, have been found. Small ones of five and six pounds are common, but also two larger ones weighed 100 and 156 pounds respectively. They were quartzites, and were found usually half embedded in the Roger coal seam, which is 500 yards above the Arley mine, recognised as the base of the middle coal measures of Lancashire. In the deep mines of the Clifton and Kearsley collieries, near Manchester, many boulders have occurred in the upper mines, sometimes in clusters and sometimes singly, and have been found in all seams, but most plentifully in the Trencherbone seam. They occur in the coal, in the roof, and sometimes embedded in both coal and roof at depths of 720, 1,050 and 1,800 feet from the surface. *They are all rounded and water-worn,* and weigh from a pound to 1,792 pounds. The last

mentioned is much the largest boulder as yet found in the coal measure. It was one of three, the smallest being about five hundredweight, found in a group 240 yards from the surface. They were not actually found in the coal, but in the blue metal above. The largest one was of grit. Another group was found in the Trencherbone seam embedded in the coal, 1,800 feet from the surface. All the boulders here mentioned came from the middle coal measure.

Some have, however, occurred in the lower coal measures at Bacup. One, six and a quarter pounds weight of granite, well rounded and globular, was found embedded in the roof of the ganister coal measure, a mountain seam 1,000 feet below the Arley mine. Two others from the same mine have the same rounded outline, one being composed of angular grains of quartz, largely impregnated with sulphide of iron ; the other, a dark grey quartz felsite, similar to rocks in the Lake District. Boulders in these lower beds are very infrequent. A small red granite boulder, with rounded contour, was found some years ago at Bacup in a thin seam in the uppermost millstone grit. Similar boulders of foreign rocks have occurred in the Leicestershire, North Staffordshire, Forest of Dean, South Wales and other English coalfields, but apparently not in the Scotch coalfield. They have also occurred abroad in the coal-beds of Saxony, Upper Silesia, Austro-Silesia, etc. The boulders from Silesia *have the same rounded outline and smoothed and polished surface as the English ones. Some of the granulite ones have a somewhat rough surface, probably due to weathering.* They all belong to ancient crystalline rocks, such as gneiss, granite, quartz, porphyry, granulite, quartzite, etc., and vary in size from one to seven pounds. A group of ten boulders is recorded as found together in a coal mine at Kattowitz, in Upper Silesia, the largest of which weighed 176 pounds, and others fifty-five and forty-four pounds. One from Kattowitz, composed of granulite, weighed 121 pounds. Weiss, who has discussed these German boulders at some length (*Transactions of the Royal School of Mines of Breslau*), argues for their having been transported *in roots of trees.* He thinks they occur more or less at the same horizon in the various coalfields and that the granulite came from the great " mass " of old crystalline silicious rocks

of Bohemia, which stretch over large tracts of the neighbouring districts of Saxony, Silesia, Moravia, etc.

In America the boulders found in the coal seams are like our own in all respects. Prof. Orton says they all agree in mineralogical characters. In the *Geology of Ohio*, vol. v., there is described a boulder, 200 pounds in weight, of metamorphic sandstone, which was taken from the thick coal at Shaunee, and seems derived from the Cambrian rocks of the Appalachian chain. In the *Report of Progress* for 1870, an account of a worn and smoothed boulder of quartzite is given which was found in the Zaleski mine. They have not infrequently been met there above the coal, sometimes in groups, sometimes imbedded in the coal, and sometimes in the shales above it. One of these, of grey quartzite from Perry County, weighed 400 pounds. " These boulders seem confined," says Mr. Stirrup, " to one horizon in the Ohio coalfield, namely, the middle Kitanning coal, and have occurred in two localities thirty miles apart." Prof. Orton (*Amer. Jour. of Science*, xliv., p. 62) says : "The largest of these boulders was 400 pounds in weight ". Prof. Bonney, speaking of the composition of these boulders, English and American, says : " They are, no doubt, paleozoic rocks, probably derived from old granitoid beds " (*Manchester Lit. and Phil. Soc.*, 7th Feb., 1888, pp. 1-18).

In regard to these detached boulders in the coal measures, which are all rounded and waterworn, they none of them bear any resemblance, so far as I can see, to true glacial débris. Not only so, but the way in which they occur seems to me to entirely preclude any ice action of any kind. They are found actually embedded in the coal and in the shales above the coal seams, in both of which we have only remains of sub-tropical and temperate plants (see *Glacial Nightmare*, pp. 434, 435), which must have been growing and must have been deposited when the stones themselves were placed there. How could a vegetation like this grow in a climate where either ice-sheets or icebergs existed, and how in the case of icebergs, to which the appeal is generally made, could the land surfaces on which grew this terrestrial vegetation, which included terrestrial animals, have been at the same time so submerged that icebergs could float over it ? The stones are

not found in beds intercalated with the coal and shales, but actually in the coal itself and in the shales, which are full of remains quite inconsistent with the presence of ice or icy-cold water, and it seems as plain as plain can be that, whatever deposited these waterworn stones, ice, in any form, is out of the question. This argument has been admirably stated by my friend Prof. Boyd Dawkins (*Trans. of the Manchester Geol. Soc.*, xix., pp. 422, 423).

Prof. Prestwich, in discussing the supposed traces of glacial action in the primary rocks generally, says : " It is not easy to admit a claim for ice action during carboniferous times, when the luxuriant vegetation of the coal measures flourished, not only here, but on Bear Island and other northern lands. With respect to the blocks of granite alluded to as occupying the lower beds of the coal measures in France, they may be, like the Tors of Cornwall, blocks left *in situ* from the decomposition of the granite on which the coal measures there rest, or they may be boulders washed down at that period by the torrents from the adjacent granitic mountains." Again, in speaking of the Devonian and Silurian beds, he says : " Although there may be at times instances in which the blocks show striæ and are derived from rocks not known in the locality, it must be borne in mind that such striated masses may be fragments of slickenside surfaces in the rocks from which the breccias are derived ; and that although a particular rock may no longer show in the locality, it may exist there buried beneath newer deposits, as, among others, in the case of the granite of the Ardennes which, although formerly unknown there, was met with in a railway cutting beneath a slight covering of paleozoic rocks " (Prestwich, *The Glacial Period, with reference to the antiquity of man*, pp. 5, 6).

Up to this point it seems plain that the traces of anything like what are called glacial phenomena are too slight and sporadic and doubtful to detain us. We now reach an horizon where such phenomena become suddenly very pronounced and where they are also found to occur in many countries, namely, the horizon which is on the borders of the so-called carboniferous and Permian beds. As I have said, I propose to analyse and discuss these permo-carboni-

ferous beds in a later page, and will now pass on to the next geological stage.

In America Prof. Shaler has endeavoured to find some traces of so-called glacial phenomena in the Jurassic rocks. Thus he would attribute the conglomerates of Jurassic age in the valley of the Connecticut, in a part of which lie the famous bird tracks, to glacial origin. This he infers from the great thickness of the beds, the absence of life from the accompanying sandstone, the rectangular forms of many of the pebbles, and from their similarity to those found in the modern drift of the region. To this Dr. Wright replies : "Upon this it is proper to remark that the drift in the lower Connecticut valley would, to a great extent, come from the same region, whether brought by ice or water, and the extent to which the pebbles would have been reduced to uniformity and smoothness by attrition depends upon the distance to which they have been rolled, or the length of time to which they have been subjected to wave action. From what appears, the evidence is not clear that the fragments from which the pebbles were made may not have originated in the near vicinity, and so their rectangular condition need not imply glacial agency in transportation."

Turning to the oolitic beds of Scotland cited by Croll, Prof. Prestwich says we must seek for some other explanation to account for the dispersion of the conglomerates and boulders in face of the incompatible fact that at those times warm conditions of climate extended to 70°-80° north, and that corals, cephalopods and huge reptiles swarmed in the seas (*op. cit.*, p. 5).

In regard to the cretaceous series, I have only to add to what I previously said that I overlooked the occurrence of large masses of gneissic and other rocks as having been found in the Cambridge greensand beds. In regard to them Prof. Hughes mentions that in the Woodwardian Museum there is *a boulder of green sandstone with shells and masses of phosphate attached and covering striations caused by weathering along lines due to rock structure. But he adds, " these are in no way glaciated rocks "* (*Trans. of the Cambridge. Phil. Soc.*, 1893, pp. 110, 111).

We now reach the tertiary horizon, of which I had much

to say in my previous book. Prof. Prestwich, recalling Croll's position that the glacial period generally so called occurred between 240,000 and 80,000 years B.C., says very truly that the effects of the greater and longer eccentricity of 980,000 and 720,000 years ago should surely have resulted in a still more intense glacial period. But there is no such evidence even in the later tertiary period " (*op. cit.*, p. 4).

In eocene times it is strange that no trace of anything like supposed glacial conditions has been found in beds other than those of the Alps. On this subject I would refer to my former work (see *Glacial Nightmare*, pp. 407-409).

Prof. Bonney, writing of this range, says " the Alps present us with some puzzling deposits. This chain is formed of a mass of sedimentary and crystalline rocks folded together. Amongst the most recent of the former is a group of deposits called the Flysch, which occurs on both sides of the chain. . . . Certain beds in the Flysch contain huge erratics. These, in one of the most noted localities, the Habkerenthal, not seldom range between twenty and forty cubic yards in volume. Five or six varieties of granite may be found among them, most of which are not known to occur *in situ* anywhere in the Alps. The rock in which the erratics are embedded is a coarse shale or imperfect slate of a dark colour, in which are beds of a breccia more or less sub-angular or of a conglomerate; these often occur in comparatively small lenticular patches, as if a load of stones had been suddenly shot down on to the muddy bed of the sea. In another locality, a short distance above Sepey, on the road to Ormond-Dessus, grits, conglomerates and breccias are interbedded with either a similar slaty rock or a dark muddy limestone. Here the more coarsely fragmental layers occasionally reach a considerable thickness, and are full of boulders, which range from two or three cubic feet in volume to nearly the size mentioned above, and a huge block now and then appears to be isolated in the mudstone. At this locality also very different kinds of rock are represented in the boulders—various sediments, schists, gneisses and granites, the last generally attaining the largest size. Here, however, the rock commonly resembles one of the Alpine granites. Evidently these beds of fragments are not

moraines ; at the same time it is difficult to understand how such huge masses as have been mentioned could be transported without the aid of ice. But the temperature in this geological period, if any trust can be placed in paleontological evidence, was much higher than it is at present, and the contemporaneous systems in England and France, where fossils are generally more abundant than in the Alpine region, give no sign of any interruption. Swollen torrents, descending as ' mud avalanches ' from rather lofty mountains, might produce such deposits " (*Ice Work*, pp. 267-8). A curious discovery of M. de Sarrasin shows that a number of the blocks of a crystalline character, including the granites, porphyries and gneisses, must have come from the southern Alps, while others resemble those of the Finsteraarhorn. The former fact is very interesting, as it shows that when the flysch was formed the central chain of the Alps, which would now form a barrier to the passage of these stones, had not been upheaved, and thus at this time the main watershed of the Alps was far to the south of its present position. Sarrasin thinks that the conglomerates owe their origin to torrents, which swept the rounded stones down to the sea. Studer thought the conglomerates and breccias were coast formations (*Great Ice Age*, p. 826).

In regard to this horizon Wallace says : " It is even more characteristically tropical in its flora and fauna ; palms and cycadaceæ, turtles, snakes and crocodiles then inhabited England. Yet on the north side of the Alps, extending from Switzerland to Vienna, and also south of the Alps, near Genoa, there is a deposit of finely stratified sandstone, several thousand feet in thickness, quite destitute of organic remains, but containing in several places in Switzerland enormous blocks, either angular or partly rounded, and composed of oolitic limestone or of granite. Near the lake of Thun some of the granite blocks found in this deposit are of enormous size, one of them being 105 feet long, ninety feet wide, and forty-five feet thick. The granite is red and of a peculiar kind, which cannot be matched anywhere in the Alps, or indeed elsewhere. Similar erratics have also been found in beds of the same age in the Carpathians and in the Apennines " (*Island Life*, 2nd edit., pp. 178, 179).

The champions of uniformity have been greatly embarrassed to find an explanation of these beds. Wallace says, quite frankly, that wherever these erratics occur they are always in the vicinity of great mountain ranges, and it is quite clear that wherever beds of the same age occur elsewhere than near mountains there are no traces of any such angular blocks and boulders as we meet in these Alpine beds.

It is also plain that the Alps were for the most part, if not altogether, uplifted during tertiary times. I know of no evidence whatever for supposing that in pre-tertiary times there was any great mass of mountains where the flysch beds occur, but, on the contrary, the vast masses of submarine beds, nummulitic limestones, etc., now at great heights, and the fact that the very beds containing the big boulders are finely stratified, make it plain that we have to do with a condition of things in which neither glaciers nor icebergs are admissible, for both of them require highlands as a gathering ground and nursery, and therefore the explanation of these beds offered by Prof. Judd is inadmissible.

One sentence in Dr. Wallace's account of the same beds is equally inexplicable to me. He argues that although the Alps have in great part been elevated during the tertiary period, we must remember that they must have since been very much lowered by denudation, of the amount of which the enormously thick eocene and miocene beds now forming portions of them are in some degree a measure as well as a proof. This logic I cannot follow. As the beds are largely submarine and stratified, it seems to me that whatever denudation they testify to is not subaerial, but subaqueous. They were not uplifted in fact at all until their layers had been deposited under the water. The fact that these stratified beds are void of organic remains does not seem to me to point to any cold conditions. Marine organisms abound profusely in the arctic seas, and as to the blocks, their size and character point to great and violent dislocations rather than to the diurnal denudation of mountains. Prestwich says the beds were laid down in a period of Alpine disturbance and change. It seems as plain as can be that whatever these Alpine eocene beds testify to, it is to their having nothing to do with ice action.

Turning from the eocene horizon to the miocene one, I would refer to my former work, pages 448-453, for ample details and arguments in regard to their not presenting any traces of glacial action, and will now add only two or three paragraphs from other writers. Thus Dr. Wallace says : " The miocene deposits of central and southern Europe contain marine shells of some genera now only found farther south, while the fossil plants often resemble those of Madeira and the southern states of North America. Large reptiles, too, abounded, and man-like apes lived in the south of France and in Germany. Yet in northern Italy, near Turin, there are beds of sandstone and conglomerate, full of characteristic miocene shells, but containing in an intercalated deposit angular blocks of serpentine and greenstone, often of enormous size, one being fourteen feet and another twenty-six feet long. Some of the blocks were observed by Sir Charles Lyell to be faintly striated and partly polished on one side, and they are scattered through the beds for a thickness of nearly 150 feet. Rock similar in kind to these erratics occurs about twenty miles distant in the Alps. It is interesting that the particular bed in which the blocks occur yields no organic remains, though these are plentiful both in the underlying and overlying beds " (*Island Life*, 2nd edit., p. 178).

Similar arguments to those above employed in the case of the eocene beds of the Alps seem applicable to the miocene beds just named (if they are really miocene and not pliocene), with the additional difficulty that in the case of the beds of the Superga and near Turin the beds are situated some distance from any possible mountains. The foot of the Alpine chain is more than ten miles from the Superga, and even if we postulated miocene peaks higher than any present mountains (which is a postulate not to be granted without proof, in view of the fact that so many of those beds are submarine), it would hardly, as Dr. Bonney truly says, account for the transport of the blocks.

But it is important to remember that tertiary beds are represented elsewhere than in mountainous and Alpine districts. If we examine the tertiary beds of Norfolk from the base of the Coralline Crag upwards there is only one spot where

anything in the shape of a foreign boulder has occurred. This is at the base of the Coralline Crag, on the southern side of Sutton Farm Hill. Mr. Prestwich, in his paper on the crags, published in the *Quarterly Journal of the Geological Society*, 1871, pp. 117-134, describes a pit now filled up. There is a bed one to one and a half foot thick, immediately above the London clay, and which contains mammalian and cetacean remains and cretaceous small pebbles of quartz and of flint, and some large pebbles of light-coloured hard silicious sandstone. In this bed was also found a remarkable rounded boulder of dark red porphyry of considerable size and weighing about a quarter of a ton. Prof. Prestwich said he knew nothing analogous to it in the rock specimens from the north of England and Scotland, and suggests as possible that it may have come from Scandinavia or the Ardennes. Oolitic remains found in the same bed probably came from central England. The important things to remember are that the boulders were rounded and therefore waterworn; that none of the specimens were angular or striated; that blocks of septaria found with them were drilled by boring molluscs; that the cetacean bones which were also found there were worn and flat and punctured on their surface, while *terebratula grandis* and *cyprina* and numbers of fragments of bryozoa were found with them, all pointing to their having been deposited in water and having nothing to do with a glacial age.

Finding it impossible to discover adequate traces of former glacial periods in the paleozoic, mezozoic and tertiary periods, Mr. James Geikie tries to outflank the position by saying that it is unreasonable to expect them. "You cannot," he says, "have broad tracts of '*inland ice*' if you have no continental areas upon which the snow and ice can accumulate." And he goes on to argue that in the earlier geological ages there were no continuous land surfaces like those of our day. Suppose that this was so, it is equally plain that the beds dating from these periods teem with relics of animal and vegetable life, and these afford most excellent thermometers, and, as Murchison and others have shown, they afford no trace whatever of severe glacial conditions having intervened anywhere in these early geological times; and we may take Mr. James Geikie's confession as conclusive that there are in

fact no adequate traces of such conditions in the earlier geological periods.

He actually says in so many words that " a remarkable uniformity of climate accompanied the insular conditions of paleozoic times " (*Great Ice Age*, new ed., p. 809), and that in mezozoic times the climate remained insular and uniform, just as it apparently was in the preceding era.

His reasoning, however, to account for the absence of glacial phenomena in the older .beds will not apply to the tertiary beds, of which we have wide stretches still remaining, showing that extensive land surfaces then existed, but with no corresponding. traces of ultra-arctic conditions except at the very end of the tertiary period.

What I urge is that the beds before the pleistocene present no traces of such deposits as mark that horizon, *viz.*, vast and continuous mantles of clay and sand, widespread erratics which have travelled for hundreds of miles, smoothed and polished striated surfaces actually present in wide continental areas. Nothing of this kind, it is admitted by Mr. James Geikie, occurs in any beds before the pleistocene. He argues that this is because it was impossible they could have been formed, while I and those who agree with me argue that they were never formed at all, because the necessary conditions were not available. These contradictory results are based on the same premise, namely, the absence of the necessary evidence.

This is not the only string in Prof. Geikie's bow, however, and I cannot quite follow him in some of his later comments. He argues that since the earlier formations are chiefly of marine origin, " it becomes evident that the records of warm and temperate conditions are more likely to be preserved than traces of cold and glacial climates ". The former, he says, will usually be represented by abundantly fossiliferous beds, while in the case of the latter the only relics that are likely to be preserved are ice-floated erratics. I cannot follow this argument. The arctic shells of the Clyde beds and Yorkshire crag were surely not more difficult to preserve than the contemporary Lusitanian shells of the English Channel; and it is not a question, as Mr. Geikie would argue, of our dealing merely with paleozoic marine débris, where we might have some difficulty in discriminating between shells

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from cold and warm seas, but with those from the whole enormous period covered by the tertiary beds, where the fauna, both subaerial and marine, has close analogies with that still living; and nowhere, neither among land animals and plants nor among molluscs, can we find the necessary evidence until we reach the term and conclusion of the tertiary age.

I cannot, again, agree with him in regard to the possibility of denudation having stripped the earth of traces of these suggested old glacial beds.· Denudation is a very ready resource for geologists in a fog, but it seems to me that if there is a class of relics which is almost indestructible, it is boulders and gravels, sands and clays. We have any number of horizons teeming with delicate marine shells and other relics. Surely they would not have escaped if contemporary boulders and polished and striated rocks had been ground away. Mr. Johnstone has some judicious remarks on the imperfection of the record as urged by Croll, Ball, Geikie and others to explain the absence of traces of glacial phenomena in the tertiary rocks corresponding to recurring cycles of eccentricity. He quotes against them the vast beds of striated conglomerates already mentioned, polished rocks, *roches moutonnées* and erratics in the rocks of permo-carboniferous age in nearly all countries in both hemispheres. If obliteration by denudation carried on over a long period were an adequate reason for the removal of all traces of glacial action in the earlier tertiary rocks of Europe, the actual preservation *universally* of abundant and undoubted phenomena, which if not glacial greatly simulate the results of glacial action, in the much more remote permo-carboniferous rocks during such a vastly greater period of time is an insoluble paradox. Thus " the imperfection of the record " and the theory of effacement as applied to the secondary and tertiary rocks utterly collapse (*The Glacial Epoch in Australasia*, p. 31).

Again, he says : " Some definite traces ought to be found intercalated among the many well-preserved beds of the equally perishable sediments of the tertiary formations within the region covered or affected by the last great spread of ice during the pleistocene age. References to the rate and amount of denudation by atmospheric and other causes, based upon the amount of sediments held in suspension

and solutions derived from the waste of the land in rivers flowing into the ocean, may be fairly correct, but surely the waste is not composed entirely of the latest formed deposits. The destruction ever going on in our rocks does not operate . so intensely upon the latest layers formed as upon particular areas when slopes and troughs favour the rapidly erosive action of the great destroyer, *water in motion;* and this action operates in vertical cuts and gashes, though the envelopes of whatever strata may be underneath, rather than in sweeping away all trace of. the most recently formed layers, many of which must occur in such situations where they were covered and permanently protected by the newer sediments in course of formation, and perhaps largely derived from the immediate waste of the very oldest rocks. Why, therefore, should we not expect fairly complete vertical fragments of ancient boulder clays, moraine stuff, erratic drifts (and in the tertiary formation, at least), as commonly as we do of contemporaneous clays, deposited gravels, lignites and sediments, especially perishable stuff, otherwise derived " (*The Glacial Epoch in Australasia,* p. 40).

In regard to the general question, Prof. Leconte writes : " Of the recurrence of many glacial epochs in the history of the earth there is as yet no reliable evidence, but much evidence to the contrary. It is true that what seem to be glacial drifts with scored boulders, etc., have been found in several geological horizons, but these are usually in the vicinity of lofty mountains, and are probably therefore evidence of *local* glaciation, not of a *glacial* epoch. On the other hand, all the evidence derived from fossils plainly indicates warm climates even in polar regions during all geological periods until the quaternary. The evidence at present, therefore, is overwhelmingly in favour of the *uniqueness* of the glacial epoch. This fact is the great objection to Croll's theory " (*Elements of Geology,* p. 577).

This view of the American geologist would be approved by the majority of European geologists. It is clear, in fact, that when we examine the evidence of the older rocks it utterly fails to support such a postulate as the recurrence of ice ages. We have here and there evidence of the occurrence of a few sporadic blocks, and at one horizon, as we

have seen, traces in several parts of the world which seem to point to the existence of local glaciers in Permian times ; but of anything at all like the vast sheets of clay and sand and gravel, mingled with innumerable boulders both rounded and angular, the stretches of polished and channelled and striated rocks, the phenomena which mark the age of drift, or so-called ice age, which extend not over a few yards or even a mile or two of rocky surface, but over whole continents, of all this or anything like it we have nothing.

It is no use prosecuting the matter further. Nothing can be plainer, both from the facts themselves and the arguments used by the champions of the glacial nightmare, that the case for a succession of glacial periods has entirely broken down. Nothing remains but hypotheses and purely imaginative begging of the question for those who profess to see in the geological record traces of former ice ages. In so far as the astronomical theory requires such a support, therefore, it utterly fails, and it is clear that on this particular issue the facts supplied by geology to the astronomer do not justify the latter in formulating an astronomical theory of an ice age.

Let us now turn to another factor involved in the astronomical theory of an ice age, namely, the distribution of the so-called glacial phenomena in regard to the two poles of the earth.

It seems very clear that if the astronomical theory of an ice age be the real explanation of that phenomenon, we should find the traces of its handiwork grouped about each pole as a focus and centre, and becoming less marked as we get away from it ; that the snow and ice ought in fact to culminate at the two poles in the form of two gigantic ice caps, as Agassiz and Croll in fact supposed, which would move southwards, carrying their loads of débris with them. But as a matter of fact the evidence shows that this was not so, as I showed at considerable length in my former work (*vide The Glacial Nightmare*, pp. 170-518).

Since writing that book further evidence has been forthcoming in regard to the very important fact that the glacial phenomena in the northern hemisphere are limited to one half only of that hemisphere, namely, that half of it bounded on the west by the Mackenzie River and on the east by the White Sea.

Krapotkin no doubt claimed to have found relics of a wide-spread glaciation in the mountains of eastern Siberia, while Czekanovski professed to find similar traces in the Sayanian Mountains. Prof. Tscherski, however, an authority of the first rank on the geology of Siberia, to which he has devoted many years of patient investigation, absolutely contests these conclusions. In his memoir on the expedition of Toll and Bunge to the Siberian islands, he says that in the mountains of eastern Siberia there are only traces, and these very slight ones, of local individual glaciers. He says that the supposed scratched blocks from the bed of the river Yenissei, near the foot of the Sayanian Mountains, and which are preserved in the museum of the Imperial Academy of St. Petersburg and in the museum at Minussinsk, are merely examples of weathering, and were so accepted by the geologists who examined them at St. Petersburg. He says that, with the exception of a small area west of the Baikal, there are not to be found on the meridian of that sea, nor in the high plateau behind it, including the so-called " Apple range," any indications of the former existence of glaciers. It will thus be seen that the position which I argued for in my last work, and which I believe is not contested by Prof. James Geikie, that there are no traces of a so-called glacial age in Siberia, is fully sustained by the latest and best information on the subject, and that the observations of von Cotta in the Altai Mountains, and of Nordenskiold along the Polar sea, are fully borne out by those of Tscherski in eastern Siberia.

If we turn to north-western America, the conclusion I urged is not only sustained but even enlarged. Prof. George Dawson, while noticing the existence of traces of the action of the old glaciers of the Cordillera as far north as the vicinity of the 63rd parallel on the 137th meridian, goes on to say of northern Alaska : " No traces of glaciation were observed by Mr. McConnell still farther north along the Porcupine River, nor by Mr. Russell farther down the main valley of the Yukon, the appearances there being, on the contrary, those of a country which had long been subjected to subaerial decay, and which had not been passed over either by glaciers or by floating ice capable of bearing erratics ".

" Further illustration of the fact that the extreme north-western part of the continent remained a land surface upon which no extensive glaciers were developed, even during the time of maximum glaciation, is afforded by the note of Messrs. Dease and Simpson as to the entire absence of boulders along the arctic coast westward from the estuary of the Mackenzie River " (" Notes on the Geology of the Northern Part of Canada," *Geol. Survey of Canada*, 1886).

Prof. Geikie, in his *Prehistoric Europe*, says : " Some geologists have supposed that the great *mer-de-glace* poured down upon Europe from the polar regions. But this is disproved by the direction of the striæ in the north of Norway, in the Shetland Islands and the outer Hebrides " (p. 205). Again, in the last edition of his *Great Ice Age*, he says : " The direction of the glaciation in the extreme north of Scandinavia, the peninsula of Kola and north-eastern Finland, demonstrates that the great *mer-de-glace* radiated outwards from the high grounds of Norway and Sweden, flowing north and north-east into the Arctic Ocean and east into the White Sea, and thus clearly proving that northern Europe was not overflowed by any vast ice cap creeping outwards from the pole, as was at one time supposed by Agassiz ". This is quite sound ; but how it is to be made consistent with an astronomical theory of an ice age to which Dr. Geikie leans I know not. Croll in this matter was more consistent than his pupil.

McGee says : " The geological evidence of extensive glaciation in arctic regions is either wanting or, at least, ambiguous and equivocal. Dall not only found bones of pliocene mammals lying undisturbed and unbroken upon the surface in Alaska, but (though an accomplished geologist) failed to find the slightest evidence of glacial action (Whymper, *Travels in Alaska*, Appendix). The bones of pliocene animals so abundantly found in northern Asia seem to be destitute of glacial markings, and are found in such positions as they would not be likely to occupy if glaciation had taken place since their accumulation. The geological age of the tree found by Sir Edward Belcher in latitude 75·32 north, longitude 92 west, is doubtful ; but there seems to be some reason to believe that its age is that of 'the forest bed' of both sides of the Atlantic (see Newbury, *Geological Survey of Ohio*, 1874, ii., part

1, p. 3 ; McGee, *Amer. Jour. of Science*, xvi., p. 339 ; *Proceedings of the American Assoc.*, xxvii., p. 196). If this be so, glaciation must have taken place in north temperate countries since the tree reached maturity. But its presence furnishes the best possible evidence that the glaciation did not extend to that high latitude. As shown by Haughton, from the geological observations of McClintock's voyage, the boulders of the arctic archipelago were carried northward (*The Arctic Seas*, Appendix, p. 368) ; and it was long ago pointed out by Sir Charles Lyell that Scandinavia, Scotland, Labrador and other elevated regions formed independent centres of dispersion of erratics during the glacial period, as could not happen if a polar ice cap occupied higher latitudes. During any glacial period, indeed, the ice in temperate regions would cut off the supply of moisture previously borne northwards, in accordance with the principles which tend to prevent the accumulation of ice in the central portions of any ice field. It is exceedingly doubtful, accordingly, whether the arctic regions were ever much more extensively glaciated than now " (McGee, *Maximum Synchronous Glaciation*, pp. 10, 11).

Colonel Fielden writes : " We find, roughly speaking, that between the 40th meridian of east longitude, passing through the White Sea, and the 160th degree of west longitude, passing through Alaska, but separated by the expanse of the Atlantic Ocean, is included all that portion of the northern hemisphere which presents us with proofs of that abnormal extension of ice which we style the glacial epoch. Great as this extension of the glacial episode was, it is only fair to point out that from the Kola peninsula, in 40° east longitude, to Bering Straits the vast tundras of Siberia, extending through 150 leagues of longitude, do not appear to present any traces of that abnormal ice episode which is so remarkably impressed on north-western Europe and the northern part of America. On this point we have the authority of the most competent of observers, Baron Nordenskiold, who, in the *Voyage of the Vega*, frequently refers to the subject, and writes : " It is certain that the ice cap did not extend over the plains of Siberia, where it can be proved that no ice age, in a Scandinavian sense, ever existed, and where the state of the land, from the Jurassic period onwards, was indeed subject to some

changes, but to none of the thorough-going mundane revolutions which in former times geologists loved to depict in so bright colours. . . . If we are correct in assuming that one half of the northern terrestrial hemisphere shows no trace of the extension of the glacial ice cap, and the evidence at our command appears to be conclusive on this point, whilst the other half does, we can hardly rely on the theory that changes of the eccentricity of the earth's orbit, even with the indirect physical agencies thereby engendered, could account for the glacial epoch in north-western Europe and North America. If the glacial epoch was due to incidences connected with the eccentricity of the earth's orbit, surely the extension of the glaciated area in the northern hemisphere would have been circumpolar and not semispherical " (Fielden, President's Address, *Norfolk and Norwich Naturalist's Society,* vol. iv., p. 163).

It would seem, therefore, to be as clear as anything can be that this particular distribution of the glacial phenomena, not merely the asymmetrical outline of the area over which they prevail, but the fact that instead of being grouped in a circular zone around the pole they are found in one half only of the circumpolar area, makes it impossible to assign the phenomena to any astronomical cause.

At all events, it is clear that in the question under discussion we have another broken geological plank in the platform of those who maintain the astronomical theory of an ice age.

I propose to close this chapter with a discussion of still another geological difficulty which stands in the way of the champions of that theory, and one which has been somewhat unfairly and disingenuously treated, namely, the length of time which has elapsed since the so-called glacial age.

When Croll and Ball wish to lead our imagination captive, they contrast the present disparity in the length of summer and winter in each hemisphere with that which prevailed when the earth's orbit was marked by its greatest possible eccentricity, which one of them calculates at thirty-six days and the other at thirty-three. This was 850,000 years ago ! I have already shown that even if we take these figures we have no tendency to produce an ice age : but let that pass. Croll, who always had the courage of his opinions, when he

first wrote on the subject boldly put the glacial period during the interval between 980,000 and 720,000 years B.C., which was not only the longest cycle marked by excessive eccentricity of the earth's orbit, but also when that eccentricity reached its highest value; and he says that on this point he had consulted several eminent geologists, and they all agreed in referring the glacial period to that epoch, the reason assigned being that they considered the later period, to be presently named, much too recent and of too short a duration to represent that epoch (see *Climate and Time*, 1879, chap. xix.).

I should like to know who these geologists were, and which of them would still avow his opinion. But unless we are prepared to accept those figures, and to put the great ice age back to the period which intervened between 980,000 and 720,000 years, what business have we to be conjuring with the figures thirty-six or thirty-three at all, and contrasting a condition of things where there was a disparity between the seasons marked by that number of days with the present disparity of seven days only?

Can a geologist be found anywhere who will have the courage and temerity to put the so-called glacial age at this stupendous distance from our own time? Is there any evidence anywhere forthcoming which would justify any such daring postulate as attributing the polishing of the rocks, the scratching of the rubbed surfaces, and the distribution of the great boulders, which are the ordinary ear-marks of the so-called glacial age, at this tremendous distance?

The geological evidence is not definite, but there is a consensus among most geologists that we cannot put back the so-called glacial period more than 50,000 years ago. This is the most liberal estimate allowed, while many students place it at a much more recent period. If we adopt 50,000 years, however, as our measure, we shall find that during this period the eccentricity has at its greatest limit hardly exceeded twice its present amount; so that instead of thirty-three days, as Dr. Ball argues, we have to deal with fourteen only, being an excess of seven over the number of days which mark the disparity of the seasons at this moment; and the question for solution is whether the transfer of seven days from our present summers to our present winters would

produce an ice age. A small calculation will show that this does not nearly reach the amount which the disparity between one winter and another often touches. The decrease in the daily temperature due to this cause would in fact be far from equal to the decrease caused by a very severe winter over a very mild one, and if it be an efficient cause we ought, *a fortiori*, to have lived through several short glacial periods within the half century that some of us have been here. Sir R. Ball, in his extraordinary book, carefully avoids mentioning the fact that the thirty-three days, which he so continually quotes, are only consistent with the glacial age being placed during the interval already mentioned. If he had stated this fact, as he ought to have done, in any frank and ingenuous examination of the problem, he would have found even fewer to listen to him than he has done.

As we have said, Croll at first adopted the view that the glacial age occurred during this period of most excessive eccentricity, but he afterwards modified his conclusion. This he professedly did on the ground that, according to his calculations based on the quantity of sediment carried down by the great European rivers, the rate of denudation since so-called glacial times has been at the rate of about a foot in 6,000 years. He accordingly changed his view, and argued that the great cycle of eccentricity to which he assigned the glacial period began some 240,000 years ago, that it lasted for about 160,000 years, and terminated about 80,000 years ago. In this cycle he included the more strongly contrasted glacial and interglacial periods.

In the first edition of Dr. Geikie's *Ice Age* he accepts these later figures of Croll's as reliable, and in the second edition as *possible*, but he nowhere ventures to appeal to a period of 850,000 years ago as the date of the great ice age.

It is strange and curious, therefore, to find Croll and Dr. Geikie dealing with the contrasted figures, seven and thirty-three (or thirty-six), as if they had any application whatever to the period between 240,000 and 80,000 years ago : which they have not. A divergence between the length of summer and winter to the extent of thirty-three days means a proportion between the lengths of the two axes of the orbit of 1 to 1·077, but during the last 300,000 years the highest point of

eccentricity reached has been 1 to 1·0569, and for Dr. Croll and Dr. Geikie the latter proportion and not the former is alone valid.

But the fact is that even these later figures of Croll's, by which he put back the glacial period to between 240,000 and 80,000 years ago, are quite repudiated by almost every serious geologist I know. Dr. James Geikie seems to have felt the force of these objections, and he falls back on the dangerous conclusion that Croll's calculations as to the last cycle of eccentricity may possibly have been excessive (*Ice Age*, p. 815). This is a dangerous appeal, since not only was Croll's mathematics a strong point, but his calculations have been gone over and verified by McFarland and others; and it is a rash and dangerous thing to state on this ground, as Dr. Geikie does, that the truth of the astronomical theory nowise depends on the accuracy of Croll's determination of the date of the glacial period, which may have commenced and ended later than he supposed.

On the contrary, it seems as plain as possible that if we are to accept the astronomical theory we must accept Croll's figures. The fact is Dr. Geikie remains virtually alone among geologists in his allegiance to the astronomical theory of an ice age. He says very frankly in the new edition of his great work, page 813, in regard to Croll's notion, that the glacial period terminated as much as 80,000 years ago. Let us test this opinion by the facts.

The American geologists profess to have found a chronometer for the date of the Ice Age in the rate of recession of some of their great waterfalls. This view has been well put by an anonymous writer : " The argument of American geologists is that it was at the definitive close of the glacial period that the Falls of Niagara commenced their work. . . . They were not then at Great Island but at Queenstown, seven miles farther down, where a limestone escarpment lifts its sheer height of 300 feet from the plain. Here the Niagara took its first plunge, and began gnawing its patient way backward to the actual site of the world-renowned cataract. The resulting gorge has been described as the largest single piece of post-glacial erosion this side of the Mississippi. That it is mainly post-glacial, and has been executed at an

approximately uniform rate, gives it a special interest and importance. The Falls of Niagara indeed constitute in themselves, in Dr. Wright's apt phrase, ' a glacial chronometer '. Much trouble has been bestowed upon its accurate rating, and repeated trigonometrical surveys since 1842 afford so sure a basis for calculation that serious error in estimating from the work done, and the time consumed in doing it, need not longer be apprehended. The result is thus stated by Dr. Wright, who accepts the results arrived at by Mr. G. K. Gilbert.

" The length of the front of the Horseshoe Fall is 2,300 feet. Between 1842 and 1875 four and a quarter acres of rock were worn away by the recession of the falls. Between 1875 and 1886 a little over one acre and a third disappeared in a similar manner, making in all from 1842 to 1886 about five and a half acres removed, and giving an annual rate of recession of about two and a half feet per year for the last forty-five years. But in the central parts of the curve, where the water is deepest, the Horseshoe Fall retreated between 200 and 275 feet in the eleven years between 1875 and 1886 (*Ice Age in America*, p. 456).

" The average rate of recession arrived at through careful weighing of these and other analogous facts is five feet per annum, or nearly a mile in 1,000 years. Hence from 7,000 to 8,000 years have elapsed since the foam of Niagara rose through the air at Queenstown, and the interval might even be shortened by taking into account some evidences of pre-glacial erosion by a local stream, making it probable that from the whirlpool downward the cutting of the gorge proceeded more rapidly than it does now. The date of the close of the glacial epoch in the United States can scarcely then be placed earlier than 6,000 B.C. For it was, we repeat, the withdrawal of the ice that set the chronometer of the falls going. Their testimony does not ' stand alone '. Prof. Winchell's observations of the Falls of St. Anthony prove the same thing. They have been well summarised as follows : ' The course of the Mississippi, as traced on the tertiary fluviatile map, shows one slight but significant deviation from its present course. Pre-glacially it followed a wide bend from Minneapolis to Fort Snelling ; now it flows

straight across the intervening eight miles to its junction with the Minnesota. On its way it leaps the Falls of St. Anthony, and the rate of their retreat since 1680, exactly determined from the observations of Father Hennequin, proves them to be about 8,700 years old. This second glacial timepiece, accordingly, which, owing to its more southerly position, was started earlier than the first, gives substantially the same reading. It has been stopped. The Falls of St. Anthony are now fixed artificially at Minneapolis'" (*Edinburgh Review*, vol. clxxv., p. 3).

"Prof. G. F. Wright obtains a similar result from the rate of filling of kettle holes among the gravel knolls and ridges called kames and eskers, and likewise from the erosion of valleys by streams tributary to Lake Erie; and Prof. B. K. Emerson, from the rate of deposition of modified drift in the Connecticut valley at Northampton, Mass., thinks that the time since the glacial period cannot exceed 10,000 years. An equally small estimate is also indicated by the studies of Gilbert and Russell for the time since the last great rise of the quaternary lakes Bonneville and Lahontan, lying in Utah and Nevada, within the arid great basin of interior drainage, which are believed to have been contemporaneous with the great extension of ice-sheets upon the northern part of the North American continent. In Wales and Yorkshire the amount of denudation of limestone rocks on which drift boulders lie has been regarded by Mr. D. Macintosh (*Trans. Vict. Inst.*, xix.) as proof that a period of not more than 6,000 years has elapsed since the boulders were left in their positions. The vertical extent of this denudation, averaging about six inches, is nearly the same with that observed in the south-west part of the province of Quebec by Sir Wm. Logan and Dr. Robert Ball, where veins of quartz marked with glacial striæ stand out to various heights, not exceeding one foot above the weathered surface of the enclosing limestone" (Upham, *Causes of the Ice Age*, pp. 9, 10).

"We do not," says Dr. Wright, "bring railing accusations against those who, from astronomical considerations, confidently speak of the close of the glacial period as an event which occurred scores of thousands of years ago; but it is important to know what other beliefs that long chronology

carries with it. If anyone chooses to believe that kettle holes can stand 100,000 years, and fill up only twenty-four feet from the apex of the inverted cone, he must run the risk of being considered credulous " (*Ice Age in America,* p. 474).

Mr. Warren Upham thus sums up the American evidence on this point: " In various localities we are able to measure the present rate of erosion of gorges below waterfalls—the length of the post-glacial gorge divided by the rate of recession of the falls gives approximately the time since the ice age. Such measurement of the gorge and Falls of St. Anthony by Prof. N. H. Winchell shows the length of the post-glacial or recent period to have been about 8,000 years ; and from the surveys of Niagara Falls, Mr. G. K. Gilbert believes it to have been 7,000 years, more or less. From the rates of wave cutting along the shores of Lake Michigan, and the consequent accumulation of sand around the south end of the lake, Dr. E. Andrews estimates that the land there became uncovered from its ice-sheet not more than 7,500 years ago. Prof. G. F. Wright obtains a similar result from the rate of filling of kettle holes among the gravel knolls and ridges called eskers and kames, and likewise from the erosion of valleys by streams tributary to Lake Erie; and Prof. B. K. Emerson, from the rate of deposition of modified drift in the Connecticut valley at Northampton, Mass., thinks that the time since the glacial period cannot exceed 10,000 years. An equally small estimate is also indicated by the studies of Gilbert and Russell for the time since the last great rise of the quaternary lakes Bonneville and Lahontan, lying in Utah and Nevada, within the arid great basin of interior drainage, which are believed to have been contemporaneous with the great extension of ice-sheets upon the northern part of the North American continent" (Warren Upham, *On the Causes of the Ice Age,* pp. 327, 328).

Mr. Upham further adds : " From this wide range of concurrent but independent testimonies we may accept it as practically demonstrated that the ice-sheets disappeared only 6,000 or 10,000 years ago. It is, therefore, manifestly impossible to ascribe their existence to astronomic causes which ceased 80,000 years ago ". (See also Macintosh, *Trans.*

Vict. Inst., 1885, pp. 73-92 ; *Q.J.G.S.,* xxxix., 1883, pp. 67-69 ; *ibid.,* xlii., 1886, pp. 527-539).

"But if the ice age in America terminated—as we seem bound to admit—less than 10,000 years ago, so beyond question did the ice age of Europe. There is no possibility of separating the course of glacial events in each continent. The points of agreement are too many ; the phenomena too nearly identical in themselves, and in their sequence. Elevation and depression of continents, the formation, retreat and second advance of the ice-sheet, the accompaniment of its melting by tremendous floods, the extermination of the same varieties of animals, the appearance and obliteration of paleolithic man, all preserved identical mutual relations in the Old and New Worlds " (*Edinburgh Review,* vol. clxxv., pp. 315-317).

Prof. Prestwich tells us in his *Geology* that in his opinion the formation and duration of the great ice-sheets in Europe and America (*The Glacial Period*) need not, after making all allowances, have extended beyond 15,000 to 25,000 years instead of the 160,000 years or more which have been claimed. In another page he speaks of the difficulty of supposing that man could have existed 80,000 years or more, and that existing forms of our fauna and flora should have survived during 240,000 years without modification or change. He again mentions the difficulty of conceiving that man could have existed for a period of 80,000 or 100,000 years without change and without progress (Prestwich, *Geology,* pp. 533, 534).

In another memoir Prestwich, referring to Croll's estimate that a period of some 80,000 years intervened between the disappearance of paleolithic man with the contemporary extinct mammalia and the advent of neolithic man, says : " Many years ago I expressed an opinion in which I am confirmed by the recent observations of American geologists, that the close of the glacial period comes to within 10,000 to 12,000 years of our own time. Not only is there nothing on geological grounds to sustain the opinion that a period of 70,000 or 80,000 years intervened between the close of the glacial period and the appearance of neolithic man, but the same conclusion is forced on us by archæological evidence alone, for it is difficult to suppose that paleolithic man, with

his stone weapons and tools, his sculptural bones and rude but not inartistic sketches of the contemporary animals, could in that long period of time have made so little progress as that exhibited by the similar surroundings of neolithic man " (*A Possible Cause for the Tradition of a Flood*, pp. 19, 20).

The same writer, converging his attention more especially upon the European evidence, says that " the acceptance of dates which place the land glaciation some 100,000 or 150,000 years back would also lead to the difficulty, even on the assumption of a rate of denudation of one foot in 6,000 years (which is that postulated by Croll), that the surface wear should have been far greater than it is. For instance, to mention only two points, could the striations on soluble rock surfaces have remained so fresh as they are, and would not the limestone rock on which the boulders of silurian rocks were left on the melting of the ice on the Yorkshire hills, show much greater wear than it actually does? These boulders now stand on pedestals raised from one to two feet above the surrounding surface-level in consequence of the dissolving away of the limestone rocks. We should look for pedestals of much greater height if the glaciation took place at the distant period involved in Dr. Croll's hypothesis" (Prestwich, *The Glacial Period, with reference to the antiquity of man*, pp. 16, 17).

Again, the same writer, speaking of the difficulty of supposing that man could have existed 80,000 years (or 200,000 if pre-glacial), and that existing forms of our fauna and flora could have survived during 240,000 years without change or modification, and after giving his reasons for believing that if man was not pre-glacial he may be termed glacial or mid-glacial, continues : " Far from claiming for him the antiquity which a term of 80,000 years would give to post-glacial man, for reasons before given, I believe that the glacial epoch, that is to say, the epoch of extreme cold, may come within the limits of from 15,000 to 25,000 years, and, that of the so-called post-glacial period of the melting away of the ice-sheet to within from 8,000 to 10,000 years. This might give to palæolithic man (supposing him to be of so-called pre-glacial age, if we may be allowed to form a rough, approximate limit, and our data are yet very insufficient and subject to

correction) no greater antiquity than perhaps about from 20,000 to 30,000 years; while should he be restricted to the so-called post-glacial period, his antiquity need not go further back than from 10,000 to 15,000 years before the time of neolithic man.

" Looking at the facts above mentioned, that most of the species of our existing land and marine fauna and flora appeared in true pre-glacial times, that is to say, in the time of the forest-bed group, and were the same as now, . . . and the difficulty of conceiving that man could have existed for a period, say, of 200,000 years without change and without progress, . . . it seems to me that a shorter estimate of time is the only one in accordance with all the conditions of the problem " (Prestwich, *The Glacial Period, with Reference to the Antiquity of Man*, pp. 16-18).

These figures and arguments may or may not be reasonable and approximately right : that is a question with which I do not wish to deal just now, and may discuss in a later chapter. What I wish to urge is that, so far as I know, every geologist who has written on the subject now repudiates the possibility of putting back the so-called glacial epoch to the age pointed out by Croll, namely, that intervening between 240,000 and 80,000 years B.C.; and in thus repudiating his chronology, which is not arbitrary but is absolutely fixed by the conditions of his hypothesis, they also repudiate that postulate itself.

In regard, therefore, to the possible date of the so-called ice age, this analysis goes to show not only that geology does not countenance an appeal to the astronomical theory, but that its teaching is quite inconsistent with it; and it is remarkable that this opinion comes to us perhaps most emphatically from the other side of the Atlantic, where perhaps the most extravagant glacial theories have prevailed. Mr. Warren Upham, the mouthpiece of such views, says that " a general opinion among American geologists sets in against an astronomical theory of an ice age because of the uniqueness and the demonstrated geological recency of the glacial period " (*Amer. Journ. of Science*, xlvi., p. 116).

The fact is that, view it as we will and from whatever side, the geological evidence cannot be equated with the necessities

of the astronomical theory of an ice age. "Geology," says Sir R. Ball in his Preface, "has appealed to astronomy to aid her in the solution of a great problem, and we are now to discuss the question which the geologists have put, and the answer which the astronomers have given." "Geology," says he in his concluding chapter, "having taught us by unmistakable evidence that ice ages have happened, it is rational to inquire into the causes by which phenomena so startling have been brought about. We find that the astronomical theory offers an explanation adequate to the effects which have been observed."

It is the pretensions involved in these statements which I have tried to sift and examine, perhaps with too great minuteness and pertinacity, and to show how invalid they are. Instead of the scientific geologist making appeals to the astronomer to find him a "tortoise" on which to plant his world of conjecture, he bids him take no heed of those who clamour for his help, and who do not speak on behalf of geology but on behalf of a scholastic figment, namely, the doctrine of uniformity as understood and taught by Lyell's followers.

Note.—The scathing criticism of Dr. Lendenfeld's observations by Helms, which I quote on page 164, makes Dr. Geikie's remark about them rather inept. He says : " It is hardly likely that an observer familiar with the *roches moutonnées* of Switzerland could have misinterpreted the evidence ". It does more, it entirely destroys Dr. Geikie's case for the glaciation of Victoria, which depends entirely on Lendenfeld's evidence.

In regard to the supposed glaciation of the coast of South Australia affirmed by Tate and Jack in 1891, Mr. Geikie very frankly says that the restriction of the phenomena to the neighbourhood of the coast line and the absence of similar phenomenon in the neighbouring Adelaide range are rather suggestive. Convincing evidence of the former presence of glaciers ought to be forthcoming in the hill valleys but have nowhere been observed. R. M. Johnstone attributes the phenomenon to stranded portions of the Antarctic ice-sheet, while Jack suggests that the Southern Polar ice-cap actually invaded the coast lands of South Australia. Well may Mr. Geikie, who is not easily frightened, say that this fairly takes away his breath. To any one who has studied the history of the marine fauna of Australia, Tasmania and New Zealand since tertiary times the notion seems in fact puerile.

CHAPTER V.

AIR, WIND AND WATER AS DENUDING AND DEPOSITING AGENTS.

" It is nearly time that common sense should be exercised to resist the fallacy that weather, frost, ice and running water (such as we now experience) could have carved out mountains, dug valleys, swept away piles of strata miles high, or strewed hills, valleys and plains all over the world with streams of loose stones (including boulders as big as a house), gravel, clay and earth. The reliance of modern geology upon such feeble and inadequate agencies to produce such enormous results may be accounted for by the fact that a leader of the New Philosophy assures us both that time is *power* and that to attribute great effects to great causes is a prejudice " (J. Murray, *Scepticism in Geology*, p. 114).

THE previous chapter was devoted to confronting the astronomical theory of an ice age with the actual facts of geology, and was an attempt to show why geologists for the most part hold that the former cannot be equated with the latter. If this be so, then (even according to the extraordinary reasoning of Dr. Geikie) it is clear that that theory has failed in its purpose, and that apart from its own inherent weakness it lacks the advantage of even a plausible explanation of a great physical difficulty.

We ought now to turn to the only other theory of an ice age which has a respectable following, or perhaps (one ought to say) any following at all, namely, the theory which prevails so largely among American geologists, *viz.*, that the glacial period was directly induced by the existence of large masses of land at a high level in high latitudes at the time when it is supposed to have occurred. This is no doubt a living theory and not a dead one, and one which does not carry the heavy load of improbabilities and impossibilities which attaches to every other suggested hypothesis. It is so far plausible that it does not traverse the elementary laws of physics. It still

has to justify itself, however, by proving that it is the true explanation of our difficulties. Did, in effect, the land in high latitudes stand at a much higher level in so-called glacial times than it stands now?

On this question I shall have a good deal to say presently. I purpose in this chapter to face a somewhat smaller issue. The American geologists allow that there are serious grounds for supposing that the level of the land in very high latitudes at the period in question was lower rather than higher than it is now, but they argue that all this evidence is answered and its effect done away with by one supreme fact, namely, that of the existence of numbers of fiords and of so-called submerged valleys in high latitudes, such as Norway, Greenland and North America. These fiords and submerged valleys, we are told (and the argument is pressed home with great zeal and courage), are consistent only with the greater elevation of the country when they were formed, and it is further urged that they date from so-called glacial times.

To this it has been answered that, supposing this hypothesis is the true explanation of the fiords, it has still to be proved that their cutting out or back was a glacial and not a pre-glacial proceeding. This, however, is a domestic quarrel among different schools of glacialists. As I claim to occupy a position of absolute antagonism to the whole glacial theory here discussed, I prefer to join issue on the main fact, and I entirely dispute the view that these fiords are or were or could be in any way the products of subaerial denudation or the results of cutting down or cutting back either by rivers or the sea. If this be so, then the whole basis upon which the Americans have built up their epeirogenic theory completely fails, for the only evidence of the movement fails too. Hence it will be seen that a discussion of the mode in which these fissures were formed is not altogether a parenthetical and irrelevant part of our subject, but a very relevant one. This again is only a part of a much larger issue, upon which geologists have fought long and ardently, and upon which I take my stand, not with the new and popular school who, running in the wake of Lyell, have far outstripped their master in every extravagance which he countenanced, but

with the old masters, Hopkins and Whewell, Conybeare, Sedgwick and Murchison. These men knew something more than geology; they were mathematicians and physicists as well, were in the habit of testing the capacity of their premises before indulging in speculation, and on this issue as on others (having the necessary facts before them) deliberately rejected as futile and contrary to experience and the laws of nature the views which are now propounded in fashionable text-books as the quintessence of wisdom. Among these opinions none has been more debated, and none is more important in view of the particular discussion we are engaged in, than the great question of denudation, its extent and causes, and to a re-discussion of this question I propose to devote some space.

The older geologists converged their attention almost entirely on the earlier geological horizons : those in which the deposits were most widespread, continuous and solid ; those also which were farthest removed from our own time, and which seemed in consequence to have the least ties with the forces which are still operating everywhere. Their tendency, consequently, was to attribute almost every phenomenon they met with to exceptional and violent causes.

Against this view Hutton and Playfair raised a revolt; a much needed and a very effective revolt, and preached the doctrine that nature's methods were virtually the same in all the ages, and that the true philosophical method of explaining the puzzles which meet us in the older rocks is to study current operations and therein to find clues and guides with which to unravel the mystery. Their teaching was taken up with vigour and great powers of rhetoric by Lyell. Lyell's own studies had been chiefly on the later deposits. It was he who in fact first effectively arranged the tertiary and the so-called post-tertiary beds, and he was especially interested in watching and noting the effects of current and normal forces, of frost and rain, of river and sea, in changing, moulding and shaping the earth's surface. He did a great and lasting service to science by what he wrote on this subject, but like all inventors of new methods he greatly exaggerated the potency of his calculus, and endeavoured to explain all the phenomena of all the ages by means of it. What he

claimed as the only necessary postulate to enable him to do almost everything that the earth's history shows has occurred, and to do it with the ordinary everyday forces of nature, working in their normal attitude, was an immense draft upon time. Given sufficient time, he argued, and the smallest dynamical causes are capable of the greatest effects. He did not carry out this view with entirely rigid logic, but he carried it a long way. It was left for his scholars to complete his work in that behalf, and among them none was more extreme than Ramsay, who once presided over the Geological Survey, and whose views still largely dominate that institution. At one of the meetings of the British Association he proclaimed that nature had never varied its methods either in kind *or in degree*, and thus enunciated that extravagant doctrine of uniformity which has brought so much discredit upon the science of geology, and which is so harshly criticised by those trained in more precise methods of reasoning.

Since Ramsay made his pronouncement there has been a certain return to better and sounder methods among the less extreme geologists. It has been urged by them as it was urged by the old masters, who had seen and discussed the grander phenomena of nature as displayed in the Himalayas, the Alps, etc., etc., that while conceding that nature produces the same effects with the same causes, there are undeniable evidences everywhere that she has not always worked with the same energy or potency, but that she has at times been much more vigorous and revolutionary than at others.

So far so good ; but this is not enough for some of us. We go much further, and we contend that under the influence of Lyell's teaching a good many effects, which have been treated by his scholars, and are still so described in their text-books, as those of slow and secular methods of nature's work, are in reality (as the old masters taught) the effects of rapid and catastrophic action. That they can only be attributed to the former by ignoring some of the necessary conditions of the problem, shutting our eyes to their plain teaching and clinging to *a priori* and transcendental postulates, and to purely deductive methods of reasoning. I can only speak for myself; but I am bound to say that in travelling in the

Alps, the Jura, or in any mountain country where the internal skeleton of the earth is exposed, I find it impossible to believe that these twisted and contorted crystalline rocks, these gashes and fissures and scarps, these pinnacles, sharp and angular in outline, these tremendous faults, these great surfaces of slickensided rocks, these gaping ravines with their entering and re-entering angles corresponding on either side, these masses of recent rocks filled with marine shells, and raised mountains high, are due to any slow process of diurnal denudation whatever, but they are rather due to the spasmodic throes and convulsions of the earth in one or more of its periods of strain.

Holding this view, which was held by the serious men of science who taught me my geology, I see no improbability, but the reverse, in the conclusion that these periods of great strain were not limited to the earliest ages of the world, but have marked its history periodically in all ages. This is at all events a probability which ought not to be unwelcome to the champions of uniformitarian philosophy in its best sense. I believe, further, that the evidence is very strong indeed, if not conclusive, that one of the periods when the internal throes of the earth were most marked and most violent was the very last period of all, namely, the so-called pleistocene age. This is, of course, very far from the orthodox view. The orthodox view is that of Lyell and his scholars, who, if they very reluctantly admit great catastrophes in early geological times, look upon the earth as having then largely exhausted its more revolutionary tendencies, and argue habitually as if since tertiary times it had been as quiescent as the moon, having merely run a normal course of slow and diurnal change, to be measured in degree as well as kind by its everyday moods. They do this very largely by invoking immense drafts upon time as a substitute for the postulate of the employment of exceptional forces. To them, appeals to limitless or virtually limitless time seem more rational than appeals to spasmodic force. I hold, on the contrary, that these appeals to limitless or virtually limitless time are purely gratuitous. I quite agree with the late Mr. John Murray when he says that " it is necessary to protest against the insatiable demands of geologists for time, or rather against

the substitution of time for proof of what they assert. They seem unconscious of the fallacy of supposing that an event which to all appearance is not happening, and which certainly has not happened within the period of the world's known history, can ever come to pass, conditions and circumstances remaining the same and no new motors coming into the field. There cannot be a more groundless assumption than that 'time is power'. A late geological president has corrected the mistake in these terse words : ' It is a question of dynamics and not of time, and we cannot accept the introduction of time in explanation of problems, the real difficulties of which are thereby more often passed over than solved ' (Prof. Prestwich's Inaugural Lecture, Oxford, 1875).

" Granted millions of years," continues Mr. Murray, "table-lands, they say, will be cut down into mountains and glens ; given 4,500,000 years and the whole North American continent will be denuded away. It cost, according to Darwin, only 300,000,000 of years to denude the Weald, and positively, if we concede only a few millions more of ' untold ages,' the present continents will all be washed into the sea ! In truth, the geologist draws bills at very long dates, which are never paid because they never arrive at maturity " (Murray, *Scepticism in Geology*, pp. 3-5).

The extent to which this invocation of time, in alliance with the forces of nature acting in their normal moods, has been carried, and the extravagant lengths to which the champions of what seems to me an utterly false theory of " uniformity," as developed by Lyell's scholars, have been pushed can be best judged by some examples.

Writing in 1862, Mr. Beete Jukes, director of the Irish Geological Survey, commits himself to the following position. " Valleys of all kinds, except one class to be mentioned subsequently among volcanoes, are valleys of simple de-nudation. They have *always* been eroded or worn by the action of moving water gradually cutting away rock from between the higher ground. . . . Even among the most violently disturbed and contorted mountain chains *the most rugged chasms and ravines can in no case be shown to have been the result of surface fracture of the ground, but are in all cases the result of the erosion of moving water, and are carved by*

some form of the action here spoken of as denudation " (Jukes, *Elements of Geology*, p. 283).

Sir A. Geikie says that it can be proved that strata *miles in thickness* have been removed bodily by the seemingly feeble action of denudation. Ramsay says that 3,800 feet have been removed from the South Wales district by denudation, and 4,000 feet from the Mendip district, and 10,000 to 11,000 feet from the vale of Towey, and 9,000 feet from the rocks between Brodrick Hill and Garth Hill.

All this may be so. Anything may be possible in the vast possibilities of Nature. Science knows no *a priori* "*non possumus*," but for those among us who try our best to trace effects to something like commensurate causes, and who cannot tolerate hypotheses not based on induction, it reads like a wild dream altogether to appeal to such portentous drafts upon time without better evidence, and is as remote from sober science as the tales of Jules Verne. The hypothesis in question was in part born of the logical necessity of supporting a spurious doctrine of uniformity not based upon induction, but upon metaphysics. It is obvious these cycles of time which have not been calendered by human pens can be and ought to be measured only by their ravages and their monuments. In applying this chronometer it is utterly misleading to converge attention upon one factor only of the problem and to put aside all the rest. We must survey the whole phenomenal effects if we are to construct a lasting theory.

It would be thought absurd, for instance, for an antiquary to attribute the ruin of some castle which he met with in his rambles to the mere effect of the gnawing tooth of time, which had meanwhile spared the bare rocks on which the castle stands and had merely covered them with beards of lichen. It is in the same fashion one should argue in regard to geological time. The historical sceptic who should refuse to believe in the existence of such magnificent Vandals as Cambyses and Cromwell and Napoleon because they are only occasional and accidental factors in human history, and who preferred to treat all the ruins attributed to them to the gently repeated footfalls of time, engaged in his perpetual tramps at one pace and with one energy, would find his lucubrations

treated as paradoxes. With fashionable geologists it is entirely otherwise—the more startling the paradox the more readily does it seem to attract those who teach and those who learn, if only the preacher claims to draw his text from the gospel of uniformity.

The various eroding and transporting agencies which have been appealed to by geologists are partially meteorological, such as frost and wind and rain, and partially the action of rivers and the sea.

The operations of frost in its various forms we shall consider in more detail in a later chapter. In regard to the other denuding agencies, their effects may be considered in two aspects—chemical or mechanical. First, they can act as solvents of more or less soluble rocks, or as corrosive agents. In this aspect their action may be described as chemical.

Dry air, especially when hot, is of course a great absorber of moisture, and thus tracts of country containing lakes or marshes, and subject to the action of dry air, will become desiccated, and the whole aspect of the country will be changed. This is probably the way in which the Sahara and the deserts of Arabia have been converted from more or less fertile districts into wastes of bare rock and moving sand. This is certainly the way in which the great stretches of black and red sand known as the Karakum and the Kizilkum deserts in central Asia have been formed, having taken the place of great inland seas. Similarly, in Peru and Utah we have the same desiccation of wide districts. But in these cases the water was impregnated with various nitrates, etc., and the drying process has covered the country with a white mantle. This desiccating process is probably at this moment converting a large part of South Africa and of central Australia from a condition of fertility to one of barrenness. All this may be described in a certain sense as involving a denuding tendency, but it is one which does not come at all near the problems we are now concerned with, and may be passed by.

Air, again, when adulterated with certain gases, as with carbonic acid, which it contains almost everywhere, and with other acids, as in large cities, has a corrosive tendency upon

certain stones, etc., and decays their surface, as is familiar enough to most of us whose lives are passed in towns. When so corroded and chemically changed such surfaces are easily disintegrated by water, and no doubt in a limited but definite way this is an effective instrument of denudation. It is so limited, however, that it is hardly mentioned by the principal champions of extravagant doctrines on the subject.

Turning from air to water, we sometimes meet with conditions which also make it either a solvent or a corroding agent. Water can in certain cases dissolve the soluble constituents of rocks or beds, and in this way lead to their denudation. That is perfectly true. I have just returned from Ireland, where every stream and river has been gorged and coloured dark brown with bog water. This means a slow and continuous washing away of the soluble portions of the bogs. In other districts there is no phenomenon more common than the colouring of water of a deep brown and rusty tint by the iron oxide which it is carrying away and redepositing, while every thermal spring has a lesson of a similar kind for us.

Pure water, according to M. de Lapparent, will at ordinary temperatures dissolve one part of carbonate of lime for every 50,000 parts of water, while the proportions in the case of silicate of alumina are as 1 to 200,000. When silex is in its gelatinous condition water will dissolve $\frac{1}{1500}$ part its own weight of it, while it will dissolve $\frac{1}{460}$ of its weight of sulphate of lime or gypsum (de Lapparent, *Traité de Géologie*, p. 313). This process of solution in the case of rock salt and of gypsum, where the water has free access to the rocks and a free flow, may give rise to considerable changes of level on the surface of the ground. De Lapparent attributes to the solution of subterranean beds of gypsum the famous dislocations of the ground which took place in the valley of Visp, in the Valais in Switzerland, in 1855. That district contains more than twenty seleniferous springs.

Where rock salt occurs, either in solid beds or in saliferous marls and clays, and where running water has access to the beds, a similar dissolution is continually occurring. The fact has been taken advantage of, as is well known, in various great salt exploiting undertakings in Cheshire and elsewhere, and the large sinkings of the ground which occur about

Northwich and Droitwich and other similar localities are a fair measure of the amount of artificial subterranean erosion that has resulted from this industry.

It is obvious that what has been here induced by the manufacture of artificial brine may take place quite naturally wherever running water has access to beds containing soluble salt. There cannot be any doubt that the large number of brine springs in Cheshire and elsewhere which have been flowing for ages must have dissolved away a considerable amount of salt in this way, and it is quite possible that some of the Cheshire meres and hollows in the ground may have originated in natural subsidencies caused by the washing away of the subjacent rock salt. In certain places, as in Mesopotamia, near the Caspian, and in the United States, a considerable amount of subterranean erosion is progressing by the flowing away of pitch, petroleum and naphtha, etc., in natural springs. Apart from its dissolving and carrying away parts of some soluble rocks, water, by combining with certain rocks, may cause them to swell and enlarge, and thus cause, if not denudation, at least very considerable local alterations in the contour of the land. This process of hydration takes place chiefly in the case of salts of iron, which are thus transformed into limonite, peroxyde hydrate and anhydrite, which is transformed into gypsum. In the latter case the bulk of the material is increased 33 per cent. by the addition of water. Near Ellrich, in the southern Hartz, the hydration has caused considerable protuberances of gypsum to manifest themselves, and Credner has given a section from Hohenburg, near Eisleben, where certain beds have been greatly contorted by the anhydration of the beds subjacent to them, while the beds below the anhydrate are undisturbed.

All these, however, are very local and very limited forms of denudation, and the changes they effect are too small to attract the attention of the greater prophets of the doctrine of denudation. Let us therefore pass on.

It is not water in its pure state to which the great champions of denudations chiefly turn, but water when mixed with some corrosive acid, and notably with carbonic acid. Water contains a good deal of free oxygen in solution, and

as it permeates the ground this oxygen forms oxides by attacking various rocks. On the other hand, organic matter in solution in water deoxidises other rocks, converting peroxides into protoxides, a process especially noticeable in iron oxides. Turning to carbonic acid, this occurs in nearly all natural waters. It is absorbed from the air, from vegetation and from other sources. There can be no doubt that water when sophisticated in this fashion is capable of dissolving and corroding certain rocks much more readily than when pure, and this is especially the case when it is heated, as we know from various thermal and mineral springs. It is possible that, when sufficiently heated and when subject to sufficient pressure, water will in fact dissolve almost any material substance, even the most reluctant.

Among these intractable substances perhaps the most remarkable is silica. The hot springs that occur in Iceland and form the so-called geysers, as is known, are charged with silica in solution and deposit it in great masses. It is the same with the much more wonderful hot springs of the Yellowstone River in America and of Rotomahana in New Zealand, which have created an entirely new form of picturesque landscape by the widespread layers of white and yellow and pink silicious sinter which they have deposited. Similar deposits occur in the Azores and elsewhere, all testifying to the solvent power of acidulated hot water upon silica. Carbonic acid can similarly dissolve silicates of lime, of potash and of soda, and the oxides of iron and manganese, which form large portions of the earth's crust. Felspar, pyroxene and amphibolite are all attacked by carbonic acid in solution. Hence we have a continuous disintegration of granitic rocks. This disintegration is familiar enough. Dolomieu called it " *la maladie du granit*," which Lyell dubbed in homely fashion " the rot ". The grains of quartz in the granite remain more or less intact *in situ* and form beds of silicious sand, but the felspar is disintegrated, and thus are formed certain alkaline carbonates which are largely washed away, leaving a deposit of aluminous silica which is known as kaolin or porcelain clay. Hence the French call this form of disintegration the *kaolinisation* of granite rocks. The amount of surface decay to which granite *in situ* is thus subjected depends apparently

on more than one element ; a very important one being the
presence of sufficient moisture. In Egypt, which is very dry,
granite surfaces retain their polish for thousands of years
almost intact, while at Petersburg it as rapidly corrodes.
The tors and exfoliated surfaces of granite of Dartmoor and
Cornwall, of Brittany and Normandy and Auvergne, are a
proof that this surface decay has been a very material one,
while the rotten state of the upper layers of this rock,
sometimes extending for several yards in depth in very ex-
posed situations, and especially in tropical countries like
Brazil and Africa, testify to the same fact. The heavy tropi-
cal rains and fierce sunshine combine no doubt to enhance
this effect. De Lapparent would explain by this process of the
decay of primitive rocks *in situ* the vast and problematical
deposit known as laterite, which occurs over such wide
districts in India and South America. The same writer
says that basaltic rocks containing basic silicates, such as
amphibolite and pyroxene, are also easily disintegrated by
acidulated damp air and are converted into carbonates such
as limonite (*i.e.*, carbonate of iron), forming layers of soft
materials on the surface.

It is chiefly, however, limestone rocks which are attacked by
carbonic acid in solution. " While," says M. de Lapparent,
" pure water will only dissolve $\frac{1}{50000}$ part of limestone, water,
when charged with carbonic acid, will dissolve $\frac{1}{1000}$ part,
showing what a comparatively powerful instrument of corro-
sion that acid becomes." The effect on the limestone rock is
to dissolve away its calcareous parts by the conversion of the
carbonate of lime into bicarbonate, leaving behind a deposit
of ferruginous clay. This deposit is always left behind, even
when the purest and whitest marble or limestone is exposed
to an acid in the laboratory, and it forms the surface layer in
all limestone and chalk districts. De Lapparent attributes
to this solvent the formation of the holes in the surface of
limestone known to the German Swiss as *karrenfelder* and to
the French Swiss as *lapiez*, while the sand pipes in chalk
districts have been similarly assigned to the same cause by
English geologists. When the rock contains magnesia as
well as lime the carbonic acid attacks the calcareous parts
much more readily than the magnesian, and in this way the

proportion of magnesia in a rock sometimes increases as we get nearer the surface, the limestone thus becoming more and more dolomitic.

Lyell mentions that near Mount Gibaud, not far from Clermont, there is a gneiss formation so permeated with carbonic acid gas, and the carbonates of iron, lime and manganese are so dissolved by it that the rock is rendered soft and the quartz alone remains unattacked.

The facts here mentioned in regard to the chemical dissolution of rocks are plain and indisputable and more or less universal. And so long as their lessons are rightly read, and they are made responsible for phenomena of similar intensity and degree as are attested by such examples as we have named, there will not be any polemic about them in these pages. What I complain of is that the geologists who converge their attention on these and similar causes of disintegration apply the lessons they have learned from them to conditions which cannot in any way be equated with them. Because I can crack a nut with my teeth it does not follow that if you give me endless time I shall be able to bite through a bar of steel, and it is indeed a mighty jump from the phenomena we have just referred to, to the stupendous denudations which have been attributed to similar causes.

Let us remember a few facts on the other side. As we have seen, the capacity of pure or acidulated water to dissolve rocks depends very largely on its temperature, and increases very rapidly as it grows hotter.

Thus, in the case of silica, it would seem that acidulated water at ordinary temperatures cannot dissolve any but a quite inappreciable amount of it, and directly silicious springs get near the surface and the water they contain gets cool they have to deposit their load, and it would seem that the silicious deposits known as sinter, which form such marked features of Iceland, New Zealand and Nebraska, are all formed of silica which has been brought from strata deep down in the earth and deposited on the surface. So far, therefore, as these deposits bear testimony, it is not to the erosion of the surface of the earth by acidulated waters, but to the piling on its surface of a large load of new materials brought from great depths below the ground.

Turning to the solution of calcareous rocks, the argument also applies, though not quite to the same extent. A great mass of calcareous as well as of silicious matter is brought up by thermal waters from the beds deep down in the ground. To give one example, Bischof says that the fifty springs near Carlsbad pour out about 800,000 cubic feet of water every twenty-four hours; according to Walchnez, this water might deposit a mass of stone 200,000 lb. in weight. These thermal waters when cooled cannot hold the same quantity of calcareous matter in solution as when hot, and consequently when they approach the surface they proceed to deposit the load of material they can no longer carry. Hence the great beds of travertine which occur in certain places where these streams of water flow. Lyell mentions some notable examples of these beds, and they are indeed familiar to us all who have travelled. Among the best known are those which occur at Clermont in Auvergne and at Chaluzet in the same department. Near the Apennines, Lyell says, "we meet with innumerable springs, which have precipitated so much calcareous matter that the whole ground in some parts of Tuscany is covered over with tufa and travertine, and sounds hollow beneath the feet. In other places in the same country, compact rocks are seen descending the slanting sides of the hills very much in the manner of lava currents, except that they are of a white colour and terminate abruptly when they reach the course of a river. These consist of calcareous precipitates from springs, some of which are still flowing, while others have disappeared or changed their position. Such masses are frequent on the slope of the hills which bound the valley of the Elsa, one of the tributaries of the Arno. . . ." Lyell also describes at length the very large deposits of a similar kind at the baths of S. Vignone and S. Filippo in Tuscany, and tells us how at the former place half a foot of travertine is deposited every year in the conduit pipe conveying the water. In the Campagna, especially at the Lake of Solfatarra and at Tivoli near Rome, are very notable deposits of the same kind—the Coliseum was in fact built of travertine; so are the deposits described by Tchihatchef in Asia Minor, while those who do not want to leave our shores may find corre-

sponding deposits on a smaller scale at Knaresborough in Yorkshire and at Matlock in Derbyshire.

The moral of all these and similar deposits of calcareous matter is that acidulated water in a large number of cases is increasing and not eroding the surface of the earth.

Precisely the same conclusion follows if we examine what is going on and has been going on in caverns and fissures, where stalagmites and stalactites are being deposited, and where instead of the acidulated water being the instrument by which the hollows are being enlarged it is actually filling them up with new materials.

In these various cases, which loom very big when we put them together, it is clear we must largely discount the disintegrating powers of acidulated water when more or less heated, or rather, we must attribute to it the very reverse of an erosionary character.

If we turn to the effects of water charged with carbonic and other acids when cold, its effect on disintegrating exposed granite surfaces and limestone fells is, as we have seen, no doubt considerable, but it has its limits, and these are very soon reached. This fact has been largely overlooked, however, and quite extravagant results have been attributed to it. To this cause has been attributed the stupendous dynamical work of the cutting out of the great fissures and the caverns which occur in limestone districts, notably such enormous caverns as the Mammoth Cave in Kentucky and the famous caverns at Le Hon in the Ardennes and of Adelsberg in Austria, not to mention our own Derbyshire and Ingleborough caves. Not only so, but also the wide dry valleys, with perpendicular cliffs on either side, such as we are so familiar with in Derbyshire, in the Jura Mountains and elsewhere, and such gaps as that at Cheddar in Somersetshire. It seems to me as probable that work of this kind on this scale should have actually resulted from the dissolution of limestone by acidulated water, as that a toothless woman, who has eaten the flesh of a peach, should proceed to chew the stone.

Let us try and find some measure of the actual amount of denudation that has occurred and is now taking place in districts where rocks are subject to subaerial influences.

Mr. Wynne quotes instances of the slow rate at which limestone weathers, and therefore the enormous time required to produce results of any moment. An inscribed slab of this rock in the interior face of the battlement of a bridge a couple of miles west of Athlone, although somewhat weathered, distinctly showed in the year 1862 a date a hundred years previous. Another limestone slab, in a very similar situation, facing the east-south-east—the "Liberty Stone," at Whitehall Bridge, near Limerick—has an inscription in raised characters, about one-eighth of an inch relieved. The stone is but slightly weathered; the inscription, except in the last figure of the date, is perfect, and the date is 1635 (*Geol. Mag.*, iv., p. 7, note 1). But we need not single out individual gravestones. Every graveyard where limestone is used is full of similar monuments proving the same fact. We may say the same of much older things even than gravestones. Take, for instance, the buildings which still remain to us in some old country, such as the limestone temples in Egypt and the marble temples of Greece.

Again, in some excellent and well-known papers by Prof. Hughes, published in 1867 and subsequent years, on perched blocks and associated phenomena, he has collected a considerable amount of evidence as a test of denudation afforded by certain pedestals of limestone on which boulders are perched in the mountain limestone of the north of England. He argues that the average amount of denudation in these exposed fells is from a foot to a foot and a half since the surfaces were exposed to subaerial denudation. This means really since the so-called glacial epoch, because, as he says, we can generally see on the pedestals under some part of the overhanging Silurian blocks the limestone, smoothed, polished and strongly furrowed and striated.

In this behalf I would quote a further and important piece of evidence from another writer: "In the county of Argenteuil, in Canada, Sir William Logan described veins of quartz cutting crystalline limestone where the striated surfaces of the former stand out from six to nine inches above the general surface of the latter, showing that the limestone has been dissolved away to that depth since the striation took place. . . . I have seen many other cases, both in

Argenteuil and Ottawa counties, where hard veins and lumps embedded in crystalline limestone and bearing the striæ are weathered out to various heights not exceeding one foot above the roughened but round surface of the limestone" (*Bull. Geol. Soc. Amer.*, i., p. 306).

I am, of course, aware that pillars with their protecting caps are much higher in certain places; but in these other cases, which have been made so much of, and to which we shall return presently, the pillars are made of earth and other soft and friable materials, and not of limestone or other hard rocks, and their manufacture is an example of water portage and not of erosive denudation. Such are the earth pillars in various parts of the Alps figured and described by Dr. Bonney, notably those at Botzen in the Tyrol and at Mount Shasta in America. I fully admit the picturesqueness and grandeur of these earth pillars, but their evidence is quite irrelevant to the present part of our case, which deals entirely with chemical denudation and not with porterage.

The evidence of other hard rocks is quite consistent with that of limestone.

Thus, in a paper on the Brimham rocks, Mackintosh shows how little effect subaerial causes sometimes have had upon the millstone grit rocks of Yorkshire since the contour of the county was settled. He quotes fissures on the walls, both sides of which correspond so exactly that the minutest grit on one side appears opposite to a similar sized prominence on the other. Though open to the atmosphere, not a particle of the grit on either side of this fissure would appear to have been disintegrated since its formation (*Geol. Mag.*, ii., p. 157). Hull has found that the millstone grit projections of the Peak district retain their unworn forms (*ibid.*, iii., p. 66).

All this is assuredly very suggestive if not unanswerable. Let us now turn from the limestone and other fells of the north to the chalk deposits of the south.

Acidulated water in dissolving chalk dissolves out a considerable portion of insoluble matter in the form of ferruginous clay and gravel which remains behind, and forms a good measure of the amount of the chalk that has been dissolved away. If we travel much in a chalk district

and examine railway cuttings, chalk pits or any natural scarps, we shall be struck by the fact that where there is no rolled or semi-rolled gravel lying on the surface there is only a very thin cutaneous covering of decalcified matter, and the chalk comes practically to the surface in an unaltered and perfectly normal condition, showing how little has gone away.

An interesting and remarkable instance of the very slight denudation which chalk surfaces suffer when exposed to the air is to be gathered from the great figures of giants and of horses, etc., which occur on the downs near Marlborough and elsewhere. These, so far as archæological evidence goes, date from prehistoric times, and although carved out on the exposed faces of the Downs, remain with their outlines beautifully preserved and intact, and showing, so far as can be judged, hardly any abrasion at all. It may be said that they have been protected by a growth of vegetation, hence the necessity for occasional "scouring," but if this be so in their case it must be much more so the case of the general surface of the Downs, which has apparently been so protected from all time and continuously.

Mackintosh says, "That rain has not altered the general contour of the chalk districts since their rise above the sea can be proved by the fact that thousands of raised beaches, many of them only a few feet in height, may be found in Wiltshire, Dorset and other counties. These beaches generally conform in inclination to the surface of the neighbouring ground. In valleys they often descend very near to the bottom, thus proving that these valleys have not been excavated since the beaches were formed" (*Geol. Mag.*, iii., p. 69).

In the face of these facts it seems almost incredible that serious geologists have actually ventured to explain as the result of diurnal subaerial denudation the exposed scarps and cliffs of chalk in some inland districts. These escarpments run for many miles along the course of the beds and flank the valleys on either side. They are very prominent as the terminal boundary of the North and South Downs, but they are especially notable not in chalk but in limestone districts, where they sometimes occur at different levels in the same

valley, one behind the other. The creation of these escarpments has been attributed, as I have said, by some to subaerial denudation, etc. This has always seemed to me one of the most fantastic of all the fantastic dreams of the Huttonian school, and was acutely and persistently challenged over and over again by Mr. Mackintosh. The bottoms of the valleys in which these escarped cliffs occur, so far as we know, are not being denuded at all at this moment, but are filling up with mould or bog-earth at different rates. The very name "Weald" shows how well covered with verdure they have been. Why we should, if we follow inductive methods, suppose that any other process has been at work here, since the country has had its present general contour, I know not. It means not only that what are now grass-covered or herbage-covered plains were once bare, but also that the whole meteorology of the country has been altered. All this is possible, but it would not strike a simple student who had not been illuminated by the religion of "uniformity" as either probable or reasonable.

But suppose we grant the possibility, how are we to get a valley denuded by ordinary subaerial erosion in such a way as to form these scarps. How are the scarps to begin. Prof. Green, in a letter in the *Geological Magazine*, has suggested a way in which they could begin, but it involves river action, and I shall refer to his explanation presently; but river action is out of the question in the case of the limestone escarpments, and is repudiated by Messrs. Foster and Topley in regard to the escarpments of the Weald. I confess that, like Mr. Mackintosh, I am puzzled beyond measure to know how such scarps could commence.

If the ground was covered with grass it must have been protected from erosion and sun by the accumulation of humus. If it was bare, how comes it that it was bare in such a district, for instance, as the Weald? We know of no bare district in this part of the garden of England, nor do we know of any erosion going on there now. Suppose, however, that we strip the whole country of its herbage, how is a differential erosion to commence under the operation of subaerial causes when the strata are all of the same degree of hardness; and why, having commenced, should it go on in one particular long

section of the valley and up to a definite line of the scarp, and there stop short ?

Again, if we got a beginning and secured some agency that would go on eroding, we should expect the scarps to present some evidence of it. If the scarps had been weathered bare we should find a continuous grass-grown talus, such as we can see under the cliffs on the coast in places where the eroding tide has somewhat retired, but no such talus exists; neither a talus of chalk nor of washed-out flint. The scarp is clean cut off, showing that since it was made it has scarcely been eroded and weathered at all.

The only evidence of an active weathering agent adduced by Mr. Whitaker is the fact that in some places at the foot of the scarp there are powerful, ever-flowing springs, containing much carbonate of lime. " Such constant taking away of matter from the chalk must," he says, " wear away that rock, and, given unlimited time, is enough to get rid of any quantity of it." We have seen what this kind of dissolution really means in regard to the amount of material removed, but apart from this, it is very difficult to understand how a long line of clean-cut continuous escarpment is to be made by a number of springs planted at intervals in this way. Mackintosh says : " Springs at intervals cause land slips, which break this continuity, and therefore tend to destroy escarpments. Rain streamlets (where they are not prevented by a covering of turf) furrow their faces, and tend to disfigure a smooth slope by a series of unsightly gutters. Frost cannot act effectively when an escarpment or slope does not already exist." Mr. Whitaker, to a certain extent, seems to admit this when he speaks of " outliers " as relics of a former escarpment, and " inliers " as signs of a future escarpment. He invests subaerial agency with a power of beginning, by making something very unlike an escarpment, and ending by ruining an escarpment, the escarpment being only a state of maximum development. The fact would appear to be that the action of the atmosphere, instead of forming a slope, tends directly to make a pre-existing slope less *escarpmented.* As the same author says elsewhere, " Among the Cotswold valleys springs have given rise to landslips, but these landslips can never tend to increase, and far less to originate, the con-

tinuous and smooth regularity of a line of escarpment. . . . It is difficult, as I have said, to realise how this form of denudation would originate a cliff at all ; but suppose the cliff was begun, how would it continue its work ? In time the fall of débris would put a stop to its progress and protect the beds from decay. In the absence of a river to remove the talus, the cliff would become a slope. If, as the subaerialists admit, rivers are incapable of maintaining the vertical walls of their channels without a continual removal of the débris, how could rain, frost or even springs wear back an escarpment in the absence of an agent of sufficient power to carry away the fallen fragments or blocks. . . . Many lines of cliffs in limestone districts present little or no débris along their bases. Some of them appear as if the agent by which they were formed had swept away all the traces of its undermining action, while under other cliffs we find deposits consisting of matter which could not have been derived from the rock above " (*Geol. Mag.*, iii., pp. 65-67).

So much for the erosion of limestone and chalk. The same results follow when we turn to the subaerial decay of other kinds of rock. Let us now proceed to the denudation of rocks other than calcareous ones.

The evidence is quite consistent, whether we examine stony monuments or the heaps and mounds of softer material. Just as in the case of limestone, we have innumerable early monuments of sandstone, or of other insoluble hard materials, the handiwork of man from the earliest ages, which have been exposed to hurricane and wind and rain, which still retain their carvings and inscriptions sharp. Take the long inscriptions on the face of the cliff at Behistun in Persia, the records and representations of the early Assyrian kings at the Nahr-el-Kelb, and the exposed carvings on the rocks, called " Hällristningar," dating from the bronze age, in Sweden, all these are assuredly remarkable cases as showing how little, more than one millénnium of years has done to erode solid materials exposed to the elements. The same or even a more eloquent story is told by the monuments constructed out of soft materials, such as the mounds of mere sun-baked bricks in Mesopotamia.

Conybeare says of the barrows of the aboriginal Britons,

" after a lapse of certainly little less, and in many instances probably more, than two decades of centuries, they retain very generally all the pristine sharpness of their outlines, nor is the slight *fosse* that sometimes surrounds them in any degree filled up. Causes, then, which in two thousand years have not affected in any perceptible manner these small tumuli, so often scattered in very exposed situations over the crest of our hills, can have exerted no very great influence on the mass of those hills themselves in any assignable portion of time which even the imagination of a theorist can allow itself to conceive; and where circumstances are favourable to a greater degree of waste still, there is often a tendency to approach a maximum at which further waste will be checked—the abrupt cliff will at last become a slope, and that slope become defended by its grassy coat of proof."

M. Elie de Beaumont says numbers of large rough stones erected in pre-Roman times are planted right in the soil; many of them have fallen and many have been overturned and broken up for different purposes, but the greater part remain standing intact. Many of them are arranged in groups, circles, etc., and they prove that not only the stones themselves but the surface of the soil has remained virtually unaltered since they were planted. So do the monuments made up of the soil itself, as the so-called camp of Attila at Châlons. Except where the soil has been artificially removed, or where the ditch and ramparts have been trodden down by cattle, the entrenchments of this vast camp have hardly suffered any change whatever during fourteen hundred years, and it looks, in fact, as if it had only been evacuated a few years. The same may be said of Cæsar's camp at Dieppe. The ramparts which are found on Salisbury Plain and at the Hill of Tara in Ireland tell the same story. The tumuli bear the same testimony whether made of soil or of mixed sand and boulders or entirely of stones, as in Westphalia. M. Elie de Beaumont quotes a number of these tumuli in various parts of the world. The Roman roads point the same lesson. They remain in many places with their pavement intact. The same is the case with furrows of ancient ploughed lands. The same writer

mentions having seen in Spain and Brittany many traces of these, showing their contours perfectly, in lands which had not been worked for centuries.

Similar evidence is derivable from the fact that very ancient trees are found rooted in the soil where as seeds they must have been planted, and which cannot have been absorbed to any extent. We must remember the age of some of these trees is to be numbered in thousands rather than hundreds of years. An old English park with its gnarled oaks and yews is a good object-lesson of the very slight degradation the general level of the land has sustained from subaerial denudation during historical times.

Prestwich, *à propos* of this issue, says : " In the forest of Fontainebleau there is an oak which is said to have sheltered Clovis. In the Ardennes there was another, the age of which was ascertained to be not less than fifteen hundred years. In England some of the yew trees on our downs are supposed to have attained the age of two thousand years. Finally, there is the remarkable case of the boab trees in the Cape Verde Islands and in Senegal, some of which may, it is supposed, be from five thousand to six thousand years old."

The only attempt known to me to meet this argument is that of Sir A. Geikie and Mr. Whitaker, the former in his well-known paper in the third volume of the *Glasgow Geological Transactions.* Sir A. Geikie has taught me a great deal, but I am bound to say that I cannot quite follow his argument here. He says he admits that a considerable number of ancient stone monuments that have been always exposed to the air still remain above ground unweathered and virtually unaffected by time, but he adds it is not fair to refer to these since they are only a tithe of what once existed, and we must consider those that are gone as well as those that remain. What can this mean? It is quite true that the English farmer has broken up and carried away a considerable number of cromlechs and other old stones to make his walls and mend his roads with, and that thus the number of the old monuments has been considerably reduced. But what has this to do with the question? Has a single stone anywhere disappeared from the action of the weather? That is the whole question. Has a single one of the great

stones, from the Pyramids of Egypt to the circle at Stonehenge, had its contour or main lines perceptibly altered in any way? If not, then surely, *cadet questio.* The British farmer is certainly not an instrument of subaerial denudation in the geological sense. Turning to the old grave-mounds, etc., which are found on the wolds, Sir A. Geikie says, "It is a mistake to suppose that they have not been affected in form and outline by the subaerial denudation". I have seen many of these remains in many parts of Europe, and I am bound to say what has often struck me has been the extraordinary way in which the smallest details remain intact. If we examine the saucer-shaped mounds on Salisbury Plain, the ramparts of Cæsar's Camp at Folkestone, the Roman Wall in Northumberland or the prehistoric mounds of Southern Russia or the United States, we shall have a measure of the very slight rate at which such denudation has actually taken place, if it has taken place at all.

But it may be said that in all such instances we have not allowed a sufficient time, and that the problem to be solved is not a historical but a cosmical one, and we have to do, therefore, not with historical but with geological time, and that the age of the pyramids is nothing compared to the time during which the elements have been dissolving the limestone cliffs and mountains. It is well, therefore, to examine evidence of another kind.

Among the relics of the so-called glacial age which have been pressed upon our attention most persistently are the smoothed and scratched surfaces still exposed to the sky, and which are supposed to retain in these marks the most irrefragable proofs of the former existence of an ice age and of its relative date. The preservation of these marks has been enlarged upon with wonder and interest in almost every geological work of the last half century. They are supposed to exist everywhere where hard rocks are found, and where the so-called ice age held sway, notably in Norway, Scotland and North America, in some places exposed to the air and in others washed by the sea. I will quote one case out of very many. Dr. Bell says: "On Portland promontory, on the east coast of Hudson Bay, the striæ are as fresh looking as if the ice had left them only yesterday. When the sun bursts

upon these hills, after they have been wetted by the rain, they glitter and shine like the tinned roofs of the city of Montreal" (*Bull. Geol. Soc. Amer.*, i., p. 308).

The continued existence of such striæ on rocks, whether covered with débris or bare, and which were impressed on them (as we are told) by glacial action, is surely almost conclusive evidence of the very slight denudation these surfaces have suffered since so-called glacial times. I cannot appreciate what Sir A. Geikie says to this kind of evidence. This is what he says : " The localities where the actual ice polish and striæ are now exposed are few compared with the area from which these fine markings have been effaced. We see the surface in all stages of decay, from rocks where, save perhaps on the large scale, all vestige of ice action has disappeared to polished and striated surfaces which remain still fresh. In many cases where these markings retain such freshness it is easy to see that they have, till comparatively recently, been protected under a covering of soil, turf, gravel or clay. In cases where the striated faces of rock have been laid bare by human agency, a few years sometimes suffices to remove the sharpness which they had when the protecting clay was removed from them. Those, for example, who remember the appearance of the striated dolomite on the Queen's Drive at Edinburgh, when that road was made about a quarter of a century ago, will find that even this brief exposure has been enough to remove the original delicacy of the lines. In the old glacier districts of the Highlands I have noticed well-marked *roches moutonnées* where the rounded form and the parallel grooves and striæ still remained wonderfully distinct, yet, on examining these bosses of granite or schist, I found that the quartz veins traversing the rock sometimes projected from the general surface a twelfth of an inch or more and retained the finer striæ, which were all obliterated from the rest of the rock." All this is most sound and good, but if it means anything it means that the denudation of these exposed rock surfaces *since the so-called glacial epoch* has been only a fraction of an inch. We should all agree with the statement that glaciated surfaces wherever found are no exception to the general law of decay, and that when exposed to the air they will weather as other surfaces will. It is also true that

polished surfaces resist denudation better than rough ones, but the question is whether the theories upon which the sub-aerial excavation of valleys in hard rocks by forces in current operation is founded can be made to survive a moment's reflection, when it is admitted that exposed surfaces of such rocks have only lost one-twelfth of an inch of *surface since the so-called glacial period.*

Mr. Whitaker, *à propos* of these ice scratches, says : " It is not enough that in *some* places the weather has not acted on rocks for a very long time, it must be shown that such is the case in most places, or, in other words, that the weather *hardly ever* wears away rocks ".

This again I cannot follow. We can only test the general law by particular cases. If we select our cases with catholic impartiality, and we find them all preaching the same sermon, we have no alternative but to listen and to follow it, unless we prefer imagination to reason and poetry to science. My contention is, that wherever we put our question the answer, where an answer is available, is the same. The glacial striæ in the Alps and the so-called glacial striæ near the sea level in Norway, the polishing of the hills and of the plains, the grave-mounds of Somerset and of Ohio, the old furrowed fields of Lancashire and Brittany, the Appian Way and the Roman Road over Blackstonedge, the pyramids of Egypt and the monuments of Copan, the cromlechs and the menhirs, Thor's mound at Upsala, the great mound at Marathon and Stonehenge, all tell the same tale. There can be no question, therefore, as to where the *onus probandi* lies. Let us, however, go on.

A similar conclusion seems to follow from the sharp-edged boulders exposed on bleak mountain tops, and supposed to date from the same so-called glacial period, which still retain all their angularities, and also from the fact that many of these boulders are poised on points in a state of more or less unstable equilibrium. Thus they have remained, apparently, since they were first planted in their present position. The existence of beds of wide-spread boulder drift in definite shapes, as drumlins, eskers, asar, etc., which retain their contour in every respect, is another piece of evidence to show how comparatively slight has been the effect of subaerial denudation since the so-called glacial age.

Again, if we examine the higher Alpine valleys, we shall find that up to a certain height they have been polished and smoothed by the glaciers which have moved through them when they were larger. Above this polished surface the face of the rocks is roughly scored and weathered. The contrast is a good measure of the very slight denudation which has taken place in these valleys since the so-called glacial age.

I will conclude this part of my case by quoting a remarkable opinion from Mr. Alfred Harker's paper on the subaerial denudation of Skye (*Geol. Mag.*, 1899, p. 485 etc.), the result of several years' work on the geological survey of those islands. After telling us that the rainfall is probably the heaviest in Britain, amounting to as much as 100 or even 150 inches at sea-level and doubtless much more among the mountains, while the district is swept during the greater part of the year by winds which rise to severe gales of several days' duration, he says—"A new comer to such a district might well expect to find decisive evidence of the continued activity of eroding and transporting agencies, but such expectation would not be realised. . . . Under existing conditions the processes of degradation are practically at a stand-still, and the total result of subaerial erosion since the disappearance of the glaciers has scarcely left its mark even on the minor details of the surface features. . . . To whatever class of phenomena we turn, we are led to the same conclusion, that in the district discussed the agents of atmospheric degradation, erosion and transportation, are at the present time almost wholly inoperative, and have accomplished only insignificant results during the whole of Post-glacial time."

This completes my analysis of the effects of meteorological denudation, and I claim to have shown that while it is quite competent to produce certain effects of a very definite and precise kind, in pitting and corroding and dissolving the surface layers of rocks, it is contrary to every form of induction to suppose that, even with the huge drafts upon time so readily appealed to by orthodox geologists, it is able to do the vast work of shaping the contour of the earth's surface which has been attributed to it.

It requires a little thought to realise over how small a part

of the earth's surface even this kind of denudation is actually possible. The greater part of the land is covered with a green mantle of grass or other vegetation, which not only protects it from subaerial denudation, but by its continual decay is constantly adding instead of taking away from the surface beds. It is only on exposed mountain tops or bare surfaces, where there is no covering of grass, that the air and rain and frost have their full play, and can wash and scrape and smooth down the rocky surface.

Mr. Murray calls attention to the fact that even at the greatest heights, where frost has its utmost sway, the mountain peaks and crags are covered with perennial lichens, which preserve the surface from further erosion. Such a coating envelops the hoary blocks of Stonehenge, and has defied the storms of at least a thousand years, and where the rocks are softer and decay, the very débris which result turn into soil and support grass and herbs sufficient to stop the destructive tendency (*op. cit.*, p. 88 *et seq.*). Nature is in this matter very considerate. Whether it is in mantling the old menhirs exposed on high fells with shaggy lichens, or the chalk wolds of Sussex and the burial mounds of Wiltshire with their mantle of protecting grass, or covering the greater part of the surface of the earth with pasture and forest, she puts a very decided limit to the disintegrating influences of frost and rain.

Let us now turn to the second mode of subaerial erosure, namely that due to the action of air and water when in motion. Wind and moving water and, as I also believe, moving ice, when acting by themselves and unfurnished with the rasping and cutting tools supplied by sand or pebbles or boulders which they carry with them, are very powerful dynamical instruments, and can exercise immense pressure and force and do very effective dynamical work, but this I hold is entirely or almost entirely as porters and not as eroders or excavators. They can and do modify the soft and movable skin of our mother earth, very effectually; transporting it hither and thither and reshaping the contour of the land, but they are insignificant if not utterly powerless as eroders and excavators of hard and tough rocks.

Take the wind, for instance. It can, no doubt, dry and desiccate lakes and marshes—as it is doing at this moment in

Central Asia—and in this way wet and plastic beds of clay and loam and even sand may become powdery and susceptible of having their surface layers blown about, and if sufficiently light, as in the case of volcanic ashes, carried by the anti-trades and by the winds, which strew the snows of Greenland with cryokonite or "ice powder," [1] and which carry the ashes of Etna and Krakatoa for hundreds of miles. But it is only as carriers and not as denuders that winds act in such cases, and the total effects are comparatively very slight. As porters they are, under certain conditions, effective enough, and when blowing as a hurricane or tornado the wind can lift up enormous objects in its whirling grasp and transport them elsewhere.

The speed of cyclones can reach forty-five metres a second, or 162 kilometres in the hour, but it is generally not more than twenty or twenty-five. Fresnel calculated that the highest pressure of which the wind is capable is about 275 kilogrammes to the square metre, but, judging from the mechanical effects which have occurred in some storms, this should be raised to 400 kilogrammes. It is certain, says de Lapparent, that in 1825, during a hurricane at Guadeloupe, a plank of wood more than two centimetres thick was seized by the wind and sent right through the trunk of a palm tree, and broken furniture was transported to Montserrat, across an arm of the sea eighty kilometres broad. Whole forests of pines were laid low by the famous hurricane which overthrew the original Tay Bridge. I myself saw a wood of silver pines, varying from four to five feet in diameter, levelled by that storm, every tree lying by its neighbour, like so many dead men. Houses and steeples have been blown down, rocks have been split, iron-work carried away, and trees have been torn from their roots, etc. All this means the application of strong pressure to objects not sufficiently stable, and the weight and size of the object that the wind can move depend largely on its force and on the extent of the resisting surface. This has been well shown in the case of

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[1] It consists of a grey powder formed of refuse of all kinds brought by the winds, and apparently also contains some cosmic dust. Its mass, it is calculated, amounts to several tons to the square mile, and it imparts a grey colour to the ice fields (Reclus, *Géographie*, xv., 69).

mixed forests, when a violent gale has selected all the top-heavy pines and other coniferous trees and has spared the deciduous ones.

A more important work is done by the wind in redistributing the soft and loose materials which it meets with, and which it can rearrange when the conditions are favourable. The wind is always charged more or less with solid particles, which it is moving to and fro—snow, dust, or even small pebbles ; and when it meets with large masses of such pulverulent matter it can drive it about, raise it into the form of snow-drifts or dunes, and thus create the billowy contour of the Sahara, or the sandy deserts of Arabia and Central Asia and Mongolia. This is quite true, but this function of the wind is, of course, limited to the areas where such soft and loose materials occur in a disintegrated form. In regard to the snow, such drifts as are made by the wind in level countries, like Siberia, disappear in the spring. On high mountains such drifts are largely the cause of accumulated snow in special places and of its being able to survive the summer sun. On the coasts of Holland and France the dunes are obviously and sometimes rapidly modified in contour by the wind shifting their position, etc., and the Dutch have introduced elaborate methods of anchoring their dunes by planting them with bent-grass. A corresponding remedy has been discovered for protecting " the Landes," in the south-west of France, from the shifting sand, which was overwhelming the country, by planting vast stretches of pine forests.

The extent and size of these dunes is in some cases remarkable enough. " On the British coast ' the sand hills,' as they are commonly called, but seldom exceed thirty or forty feet in height and are limited in extent, but the dunes in some parts of the world are far more important features. In the deserts near Khokand they rise to about 100 feet " (Bonney, *Story of our Planet*, p. 96). " In the Landes of Gascony a great many dunes exceed the elevation of 225 feet. There is one even, that of Lascours, whose long ridge, parallel to the sea shore, attains 261 feet in several places, and raises its culminating dome to a height of 291 feet. In Africa, on the low shores, where the ocean bathes the great desert of Sahara, the enormous quantity of sandy materials

that the eastern winds bring from the desert and which the west winds drive back to the interior, permit, it is said, the dunes of Cape Bojador and Cape Verde to attain an elevation of 390 to nearly 600 feet. . . . On the eastern shores of all the rivers of equatorial Brazil, which discharge themselves near the mouths of the Amazon, we see, even somewhat far from the sea, ranges of dunes from twenty-five to fifty feet high, which move incessantly, driven by the breezes of the Trade winds. . . . The highest dune in the New World is perhaps that of Morro-Melancia, near Cape St. Rock, nearly 150 feet high " (Reclus, *The Ocean*, chap. i., pp. 207, 208).

These drifting sands may also be seen and studied in the desert of Atacama, the Pampas of Tamarugal, in the plains of Texas, in the Sahara of Algiers, in the Nubian deserts, etc.

The region called the Sand Hills of Wyoming territory covers an area of about 20,000 square miles on both sides of the Niobrara river. They are composed of loose, moving sand, which is blown into round, dome-shaped hills, some of them scooped out by the whirling winds so as to resemble craters (Prestwich, *Geology*, i., p. 147). These wild winds have caused at times considerable changes in topography. In Britain, perhaps, the most remarkable case which I have myself seen and puzzled over is that of the Culbin sands on the Moray Firth, where they have overwhelmed a settled district several miles in extent in a very mysterious way, since no sand is drifting thither now. In Cornwall, I may refer to the ancient Cornish church of Peranzabulo, which has been disinterred from the loads of sand which had covered it, while on the coasts of Norfolk, about Cromer, the churches of Eccles, etc., teach a similar lesson.

At St. Pol de Léon, in Brittany, a mass of sand several metres high has been advancing upon the cultivated land since 1666, at the rate, according to M. de Lapparent, of 500 metres a year. A similar desolating invasion took place in the country south of the Baltic, between Dantzic and Pillau, in consequence of the cutting down of the forests there in the last century. " In Bermuda the Trade winds are driving the sands inland in a mass, likened by Sir Wyville Thompson to a glacier of sand, overwhelming everything before them,

destroying the vegetation, entombing houses and woods, and leaving the trunks of dead trees standing upright in the midst of the sandy waste " (Prestwich, *Geology*, i., p. 146).

In Egypt many of the temples and other buildings have been overwhelmed by the sand, while a great mass of sand is marching upon the cultivated land. In the wastes of Chinese Turkestan, recently traversed by M. Hedin, whole towns were overwhelmed in the middle ages by the sand, and are now surrendering some of their remains. All this is very true, very obvious and very elementary, and it proves what a good porter the wind is when it meets with dust and sand. It would hardly have been enlarged upon here were it not for the extravagant use which has been made of this local phenomenon (which is perfectly explainable by and consistent with ordinary physical laws, and presents no difficulties) by a famous geologist to explain what is in essence a very different kind of phenomenon, namely, the origin of the Loess. The latter problem, which, as we shall see in a later chapter, is very germane to our subject, has been explained in a fantastic way, viz., by following the pernicious practice of attributing quite transcendental powers to causes whose efficiency has been tested in comparatively small effects. Baron Richthofen has in fact attributed the origin. of this last deposit to Æolian causes. This view has been countenanced by the American geologists Pumpelly and Clarence King, and in Europe has received a benevolent patronage from a number of well-known geologists, including Ramsay, Blandford, Nehring and others, who have not apparently taken pains to realise what the theory really involves; but it has been fiercely opposed by Kingsmill and Père David (who, like Richthofen, studied it chiefly in China), and by many others. Baron Richthofen does not shrink from the full consequences of his theory. He tells us that, while gazing daily at the astounding deposits and grotesque features in the Chinese provinces of Honan and Shansi, he formed views which were not only corroborated during his further travels throughout all Northern China and in the Mongolian steppes, but which on the strength of comparative study he was afterwards able to apply with equal force to Tibet, the region of Khotan and Yarkand, and great portions of South-Western Asia as

well as to all Loess-covered regions of Europe and of the continents of North and South America (*Geol. Mag.*, 1892, p. 295). Elsewhere he tells us he explains by the same agency the *chernozem* or black earth of Russia and the *regur* of India.

On turning to his explanation of the Loess, we find him saying: " There is but one great class of agencies which can be called in aid for explaining the covering of hundreds of thousands of square miles in little interrupted continuity, and almost irrespective of altitude, with a perfectly homogeneous soil. It is those which are founded in the energy of the motions of the atmospheric ocean which bathes alike plains and hilltops. Too little weight has been granted hitherto by geologists to these agencies, and yet there is no other which has contributed in a greater measure to determine and modify the character of the surface of any portion of the ground after its emergence from the sea, and to predestinate wide regions for the existence of certain kinds of plants and animals, and for the modes of nomadic or agricultural life of mankind.

" Wherever dust is carried away by wind from a dry place and deposited on a spot which is covered by vegetation it finds a resting-place, and may be washed off and carried further away by the next rain if the ground is sloping, or it may be joined to the rock if the ground is flat or slightly inclined. If these depositions are repeated the soil will gradually grow. At the same depth, therefore, to which the deepest rootlets of the grass of to-day are descending the soil may have had its surface centuries ago. Remains of the past, such as buildings and entire cities, may in this way have been entombed by dust, provided that plants were growing on its deposits, and could secure a resting-place to all further supplies of atmospheric sediment. In regions where the rains are equally distributed through the year, little dust is found, and the rate of growth of the soil covered with vegetation will be exceedingly small. But where a dry season alternates with a rainy season, the amount of dust which is put in motion and distributed through atmospheric agency can reach enormous proportions, as is witnessed by the dust storms which in Central Asia and Northern China eclipse the

sun for days in succession. A fine yellow sediment of mea-
surable thickness is deposited after every storm over large
extents of country. When this dust falls on barren land it
is carried away by the next wind, but when it falls on vege-
tation its migration is stopped. In rainless deserts the wind
will gradually remove every particle of fine-grained matter
from the soil, though a new supply of this may constantly
be provided by the action of sand blast. . . . The dust may
travel great distances, and if the wind, during the dry portion
of the year, blows constantly in one direction, that distance
will increase, while the deposition of Æolian sediments will
be cumulative in places situated in the same direction.

" There are two classes of places where the dust of continents
will rest permanently and continue to accumulate through
ages. These," he says, " are the central areas of continents,
such as the Great Basin of North America, Persia, and Central
Asia from the Pamir to the Khingan range, and from
the Himalaya to the Altai. Here the prevailing vegeta-
tion, independently of altitude, is that of salt steppe
grass and herbs and takes hold of the dust. . . . In this
way the dust will accumulate slowly but constantly through
ages on those portions of the surface which are covered by
vegetation. In the course of time it may reach a thickness
of hundreds, and perhaps thousands of feet. . . . The land-
shells which feed on the steppe, and withdraw to some depth
underneath the surface in seasons of drought or cold, will be
entombed where they die, and the most delicate shells will
be preserved. The same will be the case with the bones of
mammals and birds living on the steppe, the dryness of the
climate preventing the decay of any organic matter, as well
as the formation of vegetable mould, which would be created
in a moist climate through the decay of the organic matter.

" In this way the deepest valleys, the wildest gorges and
the largest depressions in undrained regions may be gradually
filled up with the deposits of dust, interchanging near the
encasing slopes with the angular débris of rocks, but in-
creasing in homogeneousness of composition and structure,
and in freedom from any foreign ingredients towards the
central portion of each basin. . . . If in consequence of a
lasting change of climate such a basin should gradually be

filled with water, and an outward drainage be opened, erosion would soon furrow deep channels through the earthy deposit and expose its internal structure; the fine tubes marking the site of the roots of countless generations of plants, the remains of the shells that had fed on the grass, and the bones of the mammals that had lived on the steppe would become visible, and the earth so exposed would be what is called Loess."

Richthofen tells us that this theory of the origin of the Loess was founded upon his observations in China; he tells us further that it was confirmed by what he saw in Mongolia, namely, a steppe vegetation growing on an impalpably fine earth mixed with grains of sand and accumulations of the débris of rocks at the foot of the hill sides. Proceeding to the boundaries of the undrained region, he presently noticed shallow channels of erosion, and as he went further noticed all grades of passage, to the wildest and most grotesque landscapes, where the Loess was exposed to view in the thickness of a thousand feet. He says that the salt steppes of Central Asia are surrounded on all sides by Loess regions, and that when he returned to Europe he was irresistibly led to the conclusion that its Loess must have been formed by the same process of long continued subaerial deposition of dust on steppes as that of the Eastern Continent. The same arguments, he says, seemed to him no less valid for the Loess of North and South America. In regard to Europe he says, "Every portion of this entire region, *i.e.*, Central Europe and France, must have had the character of a steppe during a sufficient length of time to allow the deposit of Loess to be formed in at least such thickness as we observe it at present". In order to obtain the continental climate in Europe, which he allows is a prime condition, he argues that Europe's north-western boundary was then conterminous with the hundred-fathom line. He quotes the researches of Dr. Nehring on the fossil fauna of the Loess as supporting his views, and concludes that, according to his subaerial theory, two different climatic stages were required for the formation of the typical Loess regions: the first of them marked by a continental climate during which the soil accumulated, the other distinguished by an increase in the fall of rain, in consequence of which the soil was furrowed by the erosive power of

water, and the steppe basins were converted into Loess basins.

I have preferred to state Baron Richthofen's views in his own words because he once complained of my having only paraphrased them. When thus stated they certainly form a splendid specimen of deductive logic as contrasted with the logic of Bacon, to which some of us are devoted.

The first point to which I would call attention is, that we are completely at issue about the kind of surroundings which the débris of the Loess fauna show must have existed when that fauna was living. Baron Richthofen says, " The genera and mostly the species of mammals found in the Loess, or their next relatives, are known to abound at present in steppes and on grassy plains ". Is this so ? The mammoth, it has been well said, would starve in a few days on the richest Craven pasture. The elephant and his nearest relatives cannot browse upon the herbage of steppes or grassy plains. Its natural habitat is the forest, its natural food the succulent branches of trees, and we actually know, as is most familiar to Baron Richthofen, that both the mammoth and the *Rhinoceros tichorhinus* did live upon the softer portions of trees, for remains of their food have been preserved and examined. These are *very characteristic* animals of the Loess, and with them were other forest animals. When I quoted this fact in answer to Baron Richthofen, Dr. Nehring, who took up the cudgels for his distinguished friend, argued against me as if I held or had ever maintained the preposterous view that because the mammoth and the rhinoceros cannot browse upon short grasses, but need the herbage of trees and long grasses or reeds for their food, and these need a correspondingly damp climate, and that inasmuch as remains of mammoths and rhinoceroses are found from the Pyrenees to Bering Straits, therefore that there must have been unbroken continuous forests over the whole of this area. Such a view could hardly be seriously maintained by any one living within our seas. For example, we have in Ireland an area apparently well suited *a priori* to the mammoth, and yet from which remains of the mammoth are virtually absent, its bones having occurred there very sporadically. While the mammoth is virtually absent from Ireland, the megaceros had its focus

of distribution there. Such facts, which are patent to us all here, make it clear that during the mammoth epoch Ireland was an area where the conditions were unfavourable to the mammoth, and exceedingly favourable to the great deer. Perhaps its forests were too dense for the mammoth, or perhaps they were composed of trees unsuited to its food. But Ireland is a mere type at our own doors of what is well known elsewhere; and to show I am not merely coining arguments for the nonce, I must be allowed to quote two short paragraphs from one of my earlier papers : " It was long ago observed that the borders of the Baltic are much less fertile in mammoth remains than those of the North Sea, and they do not in fact seem to have occurred in some large districts bordering upon it. At all events they are not named by Eichwald as occurring in Livonia, nor are they named from Ingria or Lithuania " (*Geol. Mag.*, Decade II. viii., p. 204). Scotland and Scandinavia present similar areas, while of Germany I expressly said " South Germany, with its mountainous contour, was not well adapted to the habits of the great pachyderms, and, like the mountainous district of Siberia, is not so fruitful in mammoth remains as the more level country " (*ibid.*, p. 201). I might have even said, considering their scarcity there, that the more southern and hilly zone in Siberia virtually produces no mammoth remains. What I do maintain is that the presence of the mammoth and rhinoceros and other forest animals all over the Loess area is inconsistent with a climate like that of Mongolia. If we turn from the mammals to the molluscs in the Loess, the same conclusion is inevitable. Those conchologists who are best able to decide such a question agree that the *Helices* and other shells, especially the *Succineas* of the Loess, lived in the recesses of damp woods, and their abundance proves the conditions to have been singularly favourable to them, namely, those of a humid atmosphere and of deep shade.

Out of 211,968 shells from the Rhine Loess examined by Braun it is true there was only one brackish-water form and three sweet-water forms; *Limnæus* and *Planorbis*, with but thirty-two specimens in all. Of the rest there were 98,502 examples of two species of *Succinea*, which *is an amphibious species*, and 113,434 specimens of land shells belonging to

twenty-five species of *Helix, Pupa, Clausilia, Bulimus, Limax, Vitrina* (*Deutsch. Zeits. für die gesammten Naturwiss.*, Halle, vol. xl., p. 45).

In the Bavarian highlands Gümbel found one amphibious form (*Succinea*) and fourteen terrestrial ones (*Helix, Pupa, Clausilia* and *Bulimus*). Engelhardt, who has described the Loess of Saxony, refers to twenty-four localities whence he has examined the shells of the Loess, in which only land and *amphibious shells* are to be found, while in two only did he find the fresh-water form *Limnæus truncatulus*. Similar land shells are found in the Loess of the Danube valley of Lower Austria, Hungary, the Carpathians and Poland.

Let me add to these views that of my correspondent, Prof. Todd. Speaking of the abundance of land and the paucity of aquatic shells in the American Loess, he says : " However, some semi-aquatic species, as *Succineas* and *Helicinas*, are very abundant from top to bottom of the formation, and the decidedly aquatic *Limnea humilis* is quite abundant in the Upper Loess of Western Iowa. This is sufficient to show that the formation, so far as this region is concerned, could not have been so dry as is called for by Richthofen's theory. Even the land shells observed need a much moister region than our upland prairies in their present conditions. Some of them are not found at all away from moist banks or groves." Again, " I think no one can study the life of the Missouri Loess without seeing that it bears throughout evidence of a moist climate, the very opposite of the Baron's ideal " (Richthofen's *Theory of the Loess*, by J. E. Todd, p. 6).

Dr. Nehring disputes my reading of the mollusca found in the Loess, and quotes against me Dr. Martens. Here again I must claim that my view has the support of the facts. He refers to Dr. Todd as if he were an exceptional American witness. He is quite the reverse. I have received from two American correspondents, Profs. McGee and Elsworth Call, both experienced authorities on the Loess and its contents, an elaborate paper discussing the molluscs they have found in the Loess of Iowa, and their view is entirely at issue with Dr. Nehring's. They contend that these shells require such humid conditions that they will have it the Loess is a lacustrine deposit, and Mr. Elsworth Call tells me that the

American authorities upon the Loess, including himself, Dana, Packard, Powell, Hayden, McGee, Russell, Dutton, White, etc., all maintain the same view. I cannot concur in this lacustrine theory, at least as applied to Europe and China, but I do hold strenuously with them that a large portion of the shells found in the Rhine Loess necessitate a humid condition of things. No doubt some of the molluscs point to upland districts and to drier conditions, but these are quite a minority. M. Daubrée, who reported on 200,000 specimens from the Rhine Loess, says, "Nearly all still live in cold damp climates, and some in the Alps as high as the limits of snow". Heer says of the shells from the Upper Rhine valley, all the species except *Helix ruderata*, *H. sericea*, *H. glabella*, *H. arbustorum*, *H. subalpina*, which belong to mountain districts, and *Helix strigella*, with a wide umbilicus, which is locally limited, are either forest-snails from the region of leafy trees, or species which prefer shady moist places. M. Tournouér, a first-rate authority, speaking of the similar molluscs from the Seine valley, says : "They must have lived in the recesses of moist woods attached to leaves, to tender herbaceous plants, and to rocks where waters fell. . . . They bespeak a diffusion of European species more uniform than prevails now, with a damp and more uniform climate than now prevails." But we need not surely enlarge on this part of the subject. Any one who will take the trouble to sift out the proportion of *Succineas* found in the Rhine Loess, forming nearly one-half of the shells in it, and who is aware of the life conditions of this most common mollusc, will assuredly never assign as its *habitat* in the mammoth period dry steppes continually blown over by fierce winds. I do not dispute that the upland tracts which we know existed in Germany in so-called post-glacial times, and were characterised by their own fauna, were also the home of a certain number of *Helices*, etc., which frequent mountain slopes. This is perfectly certain, but it does not in any way affect the problem as I state it, nor does it mean the importation into Europe of the parched conditions that prevail in Mongolia, where the ground is practically bare, and where Pumpelly could say that in travelling from Kalgan to Urga, a distance of 420 miles, he only saw two trees.

I do not deny that during the deposition of the Loess and its associated deposits there were two zones of life all over " the mammoth area," an upland zone and a lowland zone. Nor do I deny that the mammoth is less frequently found in the former, as in fact it is natural it should be; but this upland zone is after all a local disintegrated one compared with its neighbour. Even in the upland tract of Central Germany, as Dr. Nehring does not deny, the remains of the mammoth are of frequent occurrence, but he surely somewhat minimises its occurrence even there. I will quote a condensed passage from that admirable store-house of well-digested facts, Mr. Geikie's *Prehistoric Europe.* He says : " The mammoth, woolly rhinoceros, reindeer, horse, ox, etc., have been recorded from the Loess of many other parts of Central Europe (*i.e.*, other than Thiede and Westeregeln). Prinzinger and Czjzek mention mammoth, woolly rhinoceros and *Cervus dama gigantea* as occurring in the Loess of Upper and Lower Austria ; Zeuschner has observed a similar fauna (mammoth, rhinoceros and *Bos priscus*) in some of the valleys of the North Carpathians ; according to Dr. Roemer, mammoth, woolly rhinoceros, *Bison priscus* and *Urus* occur in the Loess of Silesia, and Hauer and Stache state that the two pachyderms appear in association with the reindeer and the horse in the Loess of Transylvania. The same species, along with ox, characterise, according to Dr. Littel, the Loess and Lehm of Bavaria " (*op. cit.*, p. 150).

It will be seen, therefore, that even in this upland zone in Central Europe the mammoth was a by no means uncommon animal. Much more frequent are its remains in the great alluvial valleys of the Danube and the Rhine, the lesser ones of the Thames, Seine and Somme, and the broad flats of Russia and Siberia, while with species of deer, urus and bison, it also characterises the Loess of China. This being so, I claim to have been strictly right in describing the mammoth, the rhinoceros, and the accompanying wood animals as the characteristic quadrupeds of the Loess.

From a remark at the end of Dr. Nehring's paper I am not quite sure whether he considers his steppe-fauna of Central Europe contemporaneous with the valley-fauna characterised by the mammoth, the bison and the red deer.

That it was so contemporaneous we have surely ample evidence. At Fisherton Mr. Blackmore found in the same deposit of brick-earth remains of more than fifty specimens of *Spermophilus* (in thirteen cases the skeletons being perfect and lying curved in the position of hibernation), two species of Lemming and an *Arvicola ;* and with them he found *inter alia* remains of the mammoth and *Rhinoceros tichorhinus, Bos primigenius* and *Bison priscus, Cervus elaphus, Cervus tarandus* of two varieties, and *Ovibos moschatus,* together with at least three varieties of *Equus,* and remains of a fourth doubtful variety, assigned by the explorer to the ass. The remains of horses we are assured by Dr. Blackmore were especially abundant (*Flint Chips,* pp. 12-30). Here then we have a complete mixture of the two series of animals all found together and proving they were contemporaneous. The remains of horses are habitually found mixed with those of the mammoth, the bison, and the different species of deer, all forest animals. The *lagomys* occurred at Kent's Hole with all the forest animals above cited, as did the lemming at Brixham, and so we might go on. I would name one more instance only as a typical one, namely, the case of the Franconian deposits reported upon by Dr. Sandberger. The following is his list (*Prehistoric Europe,* p. 62) :—

Not yet sufficiently determined, 8 species.	*Arvicola ratticeps.*	*Ursus spelæus.*
	Arvicola gregalis.	*Bos primigenius.*
Cervus tarandus.	*Spermophilus altaicus.*	*Bison priscus.*
Gulo luscus.	*Alactaga jaculus.*	*Elephas primigenius.*
Myodes obensis.	*Arctomys (? bobac).*	*Rhinoceros tichorhinus.*
„ *torquatus.*	*Hyæna spelæa.*	

This is surely plain enough. It may, and does happen, that in certain special localities we have sometimes a deposit containing animals belonging to one or other of the classes only, but this would naturally occur occasionally and locally. At all events, it seems most clear that both classes of animal, the upland and lowland class, were perfectly contemporaneous. Dr. Nehring will not dispute that the mammoth could not browse on upland pastures denuded of wood or thick shrubs. He will not deny that, with the rhinoceros, the elk, the red deer, the reindeer and the bison, the mammoth presupposes the presence of a forest vegetation. This vegetation of shady trees and long grass or reeds must have grown very

luxuriantly in the great European valleys when the vast herds of pachyderms found pasturage there. He will not deny that at Cannstadt in the Loess of Germany, and at La Celle in France, we have in the tuffs, and at Bruhl and Mamers we have in the travertines, both contemporary as far as we know with the Loess, ample remains of these very forests, which have been minutely examined and reported upon by Saporta and others. Since I wrote my early papers Mr. Geikie has published his work on *Prehistoric Europe*, and as I find myself completely in agreement with him on this point, I will quote his condensed result of these examinations, rather than repeat those I had already printed. Speaking of the plants of La Celle, he says, "This very remarkable assemblage of plants tells a tale which there is no possibility of misreading. Here we have the clearest evidence of a genial, *humid*, and equable climate having formerly characterised Northern France" (*op. cit.*, p. 51). Again, "If the winters in Northern France were formerly mild and genial, the summers were certainly more *humid*, and probably not so hot. This is proved by the presence of several plants in the tufa of La Celle which cannot endure a hot arid climate, but abound in the shady woods of Northern France and Germany" (*ibid.*, p. 52). In regard to the flora of the tufas at Cannstadt, Saporta maintains "that it indicates a climate more equable and *humid* than at present" (*ibid.*, p. 54). Surely these facts are absolutely at issue with the theory of Baron Richthofen, and with the dry parching winds and dust storms which he invokes to explain the Loess, and I do not understand how Dr. Nehring can avoid admitting it.

If we turn from this lowland zone to the upland zone, we shall surely have the same answer to give.

Dr. Nehring postulates for his steppe region, which I call the upland region, that it was marked by an expanse of grass pastures and prairies such as are found in that nomadic paradise, the valleys around the Altai Mountains, and in the famous Nogai Steppes, where the largest cattle and the finest sheep in the world are raised on the most succulent grasses, and he urges that these llanos or grass pastures were the homes of herds of horses and wild asses, of marmots, pouched

rats and tailless hares, of jerboas and porcupines, and were invaded at times by mammoths and other beasts from a differently constituted area. I see nothing to object to whatever in this. It seems fairly to describe the kind of country to be found on the southern slopes of the Ural Mountains and the inner valleys of the Altai. But let me ask, in all seriousness, how are these grassy pastures compatible with Dr. Richthofen's theory at all? Whence, let me ask, are we to derive the calcareous dust? How is luxuriant grass to grow where dust is accumulating in the fashion he describes? The grass protects the subsoil from denudation by winds completely. How, therefore, can we derive the dust from its surface? Again, if dust buried whole carcases of mammoths and other great quadrupeds so that their bones did not decay or become weathered, or their bodies dispersed and carried off by beasts of prey or otherwise, the dust must have accumulated in great quantities : whence did it come in this way? and if it came, how did the herbage grow and the animals live?

I cannot help expressing my astonishment that Dr. Nehring should have burdened his ingenious and convincing arguments in favour of a plateau fauna in Central Europe with a theory which necessitates the importation there, not of the grass prairies of the Nogai Steppes, but rather of the bare Salt Steppes of Mongolia, with their occasional tufts of hard wiry grass and occasional bushes of the still more hard and wiry steppe-shrub, the *Lasiagratis splendens*. Even with these Salt Steppes, as we argued before, the problem seems quite insoluble if we adopt Baron Richthofen's theory, *a fortiori* in the case of llanos and prairies of luxuriant grass.

Turning from the contents to the composition and structure of the Loess, I will first quote two supplementary sentences from Baron Richthofen.

He describes pure Loess, which is the same from whatever region specimens may be taken, as "composed for the most part of extremely fine particles of hydrated silicate of ammonia, while there is always present an admixture of small grains of quartz and fine laminæ of mica. It contains besides, he says, carbonate of lime, the segregation of which gives origin to

the well-known concretions common in all deposits of Loess, and is always impregnated with alkaline salts. A yellow colouring matter caused by a ferruginous substance is never wanting." This is a very good description of Loess. The Baron, however, elsewhere goes on to say : " The Loess is the residue of all inorganic matter of numberless generations of plants that drew new supplies incessantly from those sub-stances which ascend in moisture and springs, carried in rotation to the surface. This slow accumulation of decayed matter was assisted by the sand and dust deposited through infinite ages by winds. The land shells are distributed through the whole thickness of the Loess; and their state of preservation is so perfect that they must have lived on the spot where we now find them. They certainly admit of no other explanation than that here hinted at, of the formation of the soil in which they are imbedded. The bones of land animals and chiefly the roots of plants, which are all preserved in their natural and original position, give corroborative evidence " (*Journ. Geol. Soc.*, vol. xxvii., p. 377, note).

To this view there is a fatal objection, stated very clearly by Mr. Kingsmill, who says : " Its chemical composition, consisting as it does mainly of silicates of alumina and of fine silica, in the condition of impalpable sand, does not correspond with that of the inorganic elements of plants growing on its surface. Granting, however, that the earthy carbonates and a portion of the silica could be derived from such a source, whence could the plants themselves derive these elements, but in turn from the soil on which they grew ? Lime, potassa, magnesia, iron and silica, might then, so long as the plant had access to the subjacent for-mations, or was supplied by springs from below, have been deposited in a superficial layer ; silica might even, as has been suggested, have been conveyed by the medium of dust storms ; but whence could the silicate of alumina be derived ? A superficial layer not altogether dissimilar might, as sug-gested, be formed so long as the plants had access to subjacent rocks. Once, however, removed from contact with them, these inorganic elements of the plants could only be supplied from the rock itself. Rivers are inadmissible, as their action

would have been to disintegrate, not to build up; springs, from the peculiarities of the formation, cannot rise to its surface. There is finally no known means by which these inorganic matters could have been supplied from the atmosphere. The layer formed by one generation of plants would in effect have been absorbed by the next without any addition being possible " (*Journ. Geol. Soc.*, vol. xxvii., pp. 380-81).

Again, as Père David urges, if the Loess arose as is here argued we ought to meet in other districts which are being denuded *in situ* with deposits which are composed of similar materials and are being now deposited, which is not the case.

Baron Richthofen, among the reasons he gives for holding the Loess to be a wind-blown deposit, quotes the almost exclusive occurrence of *angular grains* of quartz in the pure kinds of Loess (*Geol. Mag.*, 1882, p. 296). A similar observation has been made by the local geologists in regard to the American Loess. Richthofen's is assuredly an odd reason. Let me quote a passage from the work of a skilled observer in the field of geological physics (Prof. Bonney). He says, speaking of drifted sand in Egypt : " The grains as they are hurried forward keep on striking one against another. By this incessant abrasion their angles are gradually worn off, and they are converted into miniature pebbles. A handful of sand from the Libyan desert when examined with a lens is seen to be full of rounded grains, while in that which has never been exposed to the action of the wind, such as ordinary river sand, hardly any of these grains can be found " (*The Story of the Planet*, p. 92).

Again, Baron Richthofen accepts the theory which makes the ramifying calcareous tubes found in the Loess to be the root impressions of grasses, and bases a good deal of deduction upon it. The theory that they are so, and are not due to calcareous filtration, is at present, however, very much in need of evidence, and, to my mind, has utterly collapsed. That a succession of plants growing in one area for ages should have left no débris but these fine tubes, and that they should occur throughout great thicknesses of Loess, where it is 500 and 600 feet in depth, seems quite incredible ; and I am more than inclined to accept Mr. Geikie's tentative opinion when he says : " I am not aware that any trace of

vegetable matter has ever been found in the tubes, and the capillary structure, like the concretions, may be of inorganic origin. Chemical analyses, at all events, have shown that Loess contains little or no organic matter, which we might have expected to meet with in much greater abundance had plants given origin to the innumerable vertical pores which are so commonly present in the typical deposit of the Rhine and the Danube " (*Prehistoric Europe*, p. 237).

Again, if these tubes were mere casts of roots, we ought assuredly, in regard to a *forest* flora, to find any number of casts of *tree* roots, and roots of *herbaceous* plants, which are so easily discriminated ; but we find none of these—only the ramifying fine tubes which have been called grass roots, but which, to my mind, have nothing whatever to do with grass roots, but are due to the percolation of water charged with calcareous matter. This is further supported by the fact that they do not occur where the Loess has a dense hard structure —only where it is porous ; that they occur chiefly near the surface, and diminish in size as we go down, which is the exact result we should expect from percolating threads of water acting on a deposit highly charged with carbonate of lime, and inconsistent with a deposit gradually growing higher, while the plants, whose roots the tubes are supposed to be, were continuously being buried by additions of wind-borne dust. In regard to the features just named, Mr. Todd (who, by the way, does not question their being root-marks) says : " They may be said to vary inversely as their distance below the surface. Near the surface, besides being most abundant, larger ones are found. At the depth of thirty or forty feet they are very minute and rare. None have been found lower than about forty-five feet, although several favourable localities have been examined " (Todd, *op. cit.*, p. 8).

The concretions that occur in the Loess add very considerably to the weight of evidence which makes these tubes to be the results merely of percolating water, for these concretions are clearly the result of the same agency acting slightly differently. In regard to these concretions, I shall be pardoned for quoting a graphic description by my correspondent, Prof. Ellsworth Call. He says : " They assume all possible shapes

from the spherical through the spheroidal to the oblong; in all cases they are more or less numerously studded with roughened projections. No one shape seems to obtain more than another, and not unfrequently several are found cemented together, forming an eccentric single mass. They are certainly characteristic of the Loess, for that formation nowhere occurs without their presence. *They are decidedly hydraulic, as would be naturally inferred from their constitution.* In no case have I ever observed fossils—either molluscs or vegetable matters—acting as a nucleus. On one occasion 2,803 of these bodies were crushed with that especial point in view. In nearly every instance, 2,789, they were found to contain loose fragments broken by some means from their inner walls, but no foreign substance whatever could be detected. In the remaining fourteen specimens, while the concretions were hollow, they yet contained loose particles of no substance whatever. Not a single specimen was solid throughout. *That they were originally solid, or of a pasty consistency, is not to be doubted, as a study of the inner surface reveals. They all present a deeply fissured interior consequent on the evaporation of water and subsequent contraction.* In the vast majority of cases the pyramidal masses of the interior showed distinct irregularly concentric lines of growth, or rather of accretion. . . . Prof. J. D. Whitney says of them that they ' *have been formed in the Loess by infiltration along the lines of cleavage and resultant chemical action on calcareous matter occurring in large quantity along certain planes* ' " (" The Loess of North America," *American Naturalist*, May, 1882, pp. 373, 374). To return to the tubes. I see no evidence whatever in them to support the wind theory of Prof. Richthofen, nor do I see any either in the peculiar quality of the Loess, by which it cleaves in perpendicular faces, which is no doubt due to the presence of calcareous matter in excess, and to the presence of these very tubes. Wherever we can trace current and unmistakable wind formations, such as dunes, etc., we have no such cleavage properties, and I cannot see how Baron Richthofen proposes to connect them with his predicate.

But apart from the reasons I have urged against the capillary tubes being treated as casts of roots, upon which a good deal of the reasoning in regard to the vegetable débris

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in the Loess depends, we must remember that the various analyses that have been made, especially of the American Loess, show that there is hardly a trace of carbonaceous matter in it—assuredly a very strong proof that the amount of material due to decayed vegetation in the Loess is hardly appreciable. This is true also of the black earth of Russia, and is in very marked contrast with the beds of real humus intercalated with the Loess in certain places (see Aughey, *op. cit.*, p. 176).

No doubt in Mongolia we have a high plateau covered with thin grasses, and no doubt thin grasses will arrest moving dust. This is a mere parallel to the office performed by the various silicious grasses in forming the dunes and sand hills of Holland; but the sand itself in Holland is ready made, and, as I understand Baron Richthofen, the surface of the Gobi is also formed of Loess ready made: the grass merely arrests it in certain places when it is moving, it does not help to make it. What we want to know is, how and where it was or is being made either formerly or now.

Again, the Loess for the most part is completely unstratified. Occasionally, especially in America, there are local areas where a kind of stratification occurs, but these are very local, and I shall return to them presently. This absence of stratification is assuredly a proof that it is not due to gradual accumulation by the wind. Dunes accumulated by the wind are so easy to study that we have no difficulty in finding materials, and assuredly they present quite a different structure to Loess. Deposits made by wind, especially when made as Baron Richthofen suggests, in dry seasons alternating with wet ones, have a laminar structure corresponding to the series of layers deposited, just as deposits made by water have. Nor should we find homogeneous masses several hundred feet thick with the same structure and the same contents as the results of such a *series* of seasonal deposits.

As illustrating this position I should like to quote the views of two experienced geological friends of mine. Prof. Bonney says: "Dunes frequently exhibit a regular stratification of their materials, and false bedding can be produced by wind no less than by water; as a rule both these structures are found in sand hills. On the Picardy coast they can be seen

from the train by the traveller bound for Paris from Calais. Like structures often occur on the shores of Britain, and are, no doubt, universal, but they are sometimes a little difficult to find, because the last layer of loose sand hides all beneath it " (*The Story of the Planet*, pp. 100, 101).

Speaking of the blown sands which form dunes, Prestwich says : " On these blown sands ripple marks, resembling those on a sea shore, are constantly formed, while successive layers of sand cover up the scant 'bast' grass. . . . False lamination of the sand beds is also produced by the shifting of the hillocks, like that caused by the shifting of sand shoals in the sea bed. . . . The sand dunes of Denmark described by Forchhammer show ripple marks which are not to be distinguished from those formed by the waves on the adjacent shore. The ridges of these ripples consist of the white particles of the sand, while black particles of titanic iron accumulate in the intervening depressions. . . . Dr. Forchhammer remarks how difficult it might be in case such a deposit were submerged and consolidated to distinguish it, with its ripple marks, oblique lamination and organic remains, from any ordinary geological formation accumulated under water on a shore line " (Prestwich, *Geology*, i., pp. 146, 147).

Prof. Aughey speaks of this wind-structure in some of the Loess hills on the Logan, Elkhorn, Loup, and Republican rivers. He says : " This structure is often found there as distinct as among the shifting sands of our sea coast. In every case, however, where I examined this structure in the Loess, I found it to be superficial. Out of nineteen such hills none of them possessed this structure over ten feet deep, and few of them over five feet, and many of them only from two to three feet deep. In the deep cañons, where the Loess is exposed vertically for one hundred feet, I have never found this wind-structure over ten feet deep. It occurs, therefore, only in the Loess that has been recently modified by winds, and long after it was first deposited " (*Sketches of the Physical Geography, etc., of Nebraska*, p. 274). It would be difficult to find a more striking proof that the Loess was not originally distributed by the wind than the fact that it so readily assumes a wind-structure, and yet that this peculiarity is only traceable in the superficial layers where the current winds have acted upon it.

The main masses of the Loess, to my mind, bear, on the contrary, unmistakable evidence in their very structure of having been deposited by one great effort, and under one set of conditions. Again, wind acting upon dust or sand deposits it with a very well-marked contour in dunes and sand hills, especially when the sand is arrested by grasses (*a fortiori* by forests of trees), and forms great masses of rolling sand hills on the weather-side of any area subject to sand drifts. Where are such phenomena to be traced in the structure of the Loess deposits? No doubt, in certain localities where the wind has recently and is even now disturbing the surface layers of Loess, we get such wind structure; but this is purely local, and to be explained as just mentioned.

Again, wind driving dust and sand in a definite direction, as, for example, from the Mongolian steppes towards China, would (in crossing such a broken district as the long tract of mountains bordering the valley of the Yellow River on the north) leave unmistakable traces of its passage. It would strip the high ground completely and choke up the valleys, especially those parts of the valleys under the lea of the mountains. Nothing of this sort, if we are to follow the careful observations of Père David and others, occurs; but the Loess is generally deposited in a mantle (as one American writer says) like a blanket, washed evenly over the surface, and not piled up in drifts in those places where the force of the wind could not move it. Again, such a wind would assuredly sift the materials, dropping the heavier ones first, and carrying the lighter ones further away; but this is not what we find. The texture is the same throughout and throughout, heavy grains of quartz and particles of mica occur confusedly among the finer silicious dust, as Baron Richthofen himself allows.

Again, the fact that the Loess is deposited with singularly uniform composition and structure, independently of the nature of the subjacent rocks, shows that, if *in situ* and not largely transported from areas where different materials prevail, it is entirely different from other subaerial soils which partake of the local characteristics of the rocks on which they rest. If the wind has accumulated the Chinese Loess, for instance, it must have made it as well as transported it,

for we can find no matrix *in situ* whence it could have eroded it, and how could the wind make it without being supplied with the necessary materials? Even if this explanation were plausible as an explanation of the Chinese Loess, how are we to explain the European and North American deposits of Loess. Whence did the wind bring them? They were clearly not made *in situ* by the wind acting on the drift deposits and gravels underlying the Loess. Whence then did they come?

If we turn east, west, north, or south of the great area of European Loess, whence are we to derive the dust which formed its basis according to Baron Richthofen? Wind blowing over grass pastures takes no dust—it must be over bare ground; but where are we to find such anywhere? Again, if all the grass of the Russian or French plains was taken off, Baron Richthofen will not argue that wind blowing over the ordinary loam known as diluvium or over the *chernozem* would carry off dust which, when deposited, would be Loess. All this surely demands some answer, as does the cardinal problem, that the Loess is a limited deposit in Europe with sharp boundaries, especially towards the north. I can nowhere find an answer to this critical question in Baron Richthofen's paper. Suppose we could find such a source for the dust, still our difficulties would not be ended; for the problem, as solved by Baron Richthofen, requires that we should have quite exceptional conditions. Take, for example, the demands upon our credulity involved in the following sentence in his own paper: " The Loess-covered portions of Europe extend, as is well known, from the Pyrenees, the Alps, and the Balkans in the south, to Belgium, the North German plains and Poland in the north, and from southern France in the west to beyond the limits of the continent in the east. Every portion of this entire region must have had the character of a steppe during a sufficient length of time to allow the deposit to be formed in at least such thickness as we observe at present " (*op. cit.*, p. 301). Where have we any evidence to support such an extraordinary postulate? Where is the evidence that this large portion of Europe was a dry grassy steppe when the *succineas* and other damp-loving molluscs were living here? These and the débris of plants in the tufas are very much better tests

of the climatic conditions than hypothetical appeals to a
continental climate induced by Europe being prolonged as
far as the hundred-fathom line. The presence of sea
shells mingled with those of the land, and with mammoth
remains, at the mouth of the Somme, and at several points
on our own coasts, proves such a postulate to be out of
the question at this particular epoch. In Europe, there-
fore, so far as we know, there is no area whence the dust
could come, and there is abundant evidence that the dry
steppe climate of Baron Richthofen is virtually out of the
regions of possibility. But he makes still greater demands
on our faith when, not content with speaking of a steppe
climate, he speaks of such steppes as if they were the equi-
valents of or had any analogy with the Siberian tundras, and
treats the two as if they were the same thing. The tundras
are as different to steppes as anything can be; they are
covered with thick moss, and can neither be denuded by
winds nor have their substance increased by them. They
are essentially exceedingly humid, and quite different to the
dry areas he otherwise speaks of. Surely we require some
explanations of these extraordinary statements. Here let
me say parenthetically that Baron Richthofen cannot be
serious in urging that the proof of the identity of the
pleistocene fauna of Europe, including that of the Loess,
with that buried under the tundras, upon which the theory
of a European steppe climate is largely based, was reserved
for Dr. Nehring. Has he forgotten the name of Cuvier, to
select only a very big name from a large crowd, who proved
this elementary position long before Dr. Nehring was born?
Nor assuredly can he be serious in supposing that when the
fig, the *Cyrena fluminalis*, and the hippopotamus lived in
Europe, the climate here was a steppe climate or one like
that now current on the lower Obi and Yenissei. If he
really urge this, in view of all the facts and the matured
opinions of the Russian naturalists which I have brought
together elsewhere, then I have nothing more to say.

Baron Richthofen postulates two climatic conditions as
having succeeded one another in regions where Loess pre-
vails, one marked by extreme dryness, the other by great
damp. How can this be shown when both the fauna and

flora point to a damp climate having existed at the time when he demands a very dry one? But apart from this, we are surely reasonable in asking for some foundation upon which to rest such meteorological revolutions in such areas as China, the valley of the Mississippi, and Central Europe, as he speaks of. It is easy to postulate a humid climate following a continental one, in which the conditions were almost the reverse of each other, so long as we limit ourselves to possibilities of *thought;* but we, who are morbidly anxious for some reasonable explanation of our difficulties, must have something more than transcendental predicates. We must have inductive ones. I know that local dust storms in North China do prevail, as they prevail in Mongolia and in North America, where the Loess exists; but, as Prof. Call says, the evidences of such action. are purely local, and dust storms merely tend to rearrange the surface, denuding the windward, and covering more deeply the leeward, bases and sides of the hills; but these winds.do not help us to explain either the origin of the Loess or its general distribution.

There is no evidence that the dust from the Mongolian steppes is now adding to the Loess which occurs in China, which is separated from Mongolia by the well-known chain of mountains that runs north of the Yellow River. But suppose it were, we should be no nearer solving our problem; for Baron Richthofen urges that the Loess is not being stripped from Mongolia, but is growing there too, as it is in China. How, again, can we understand a patch of steppe, in the centre of a vast area of Loess, supplying all the country round with dust, while it is being stripped itself by all the winds of heaven and while the dust is growing *in situ?* for we must remember that the Russians have shown that in the province of Irkutsk, north of the Gobi Desert, Loess occurs as it does in China. The problem of deriving the main body of the American Loess from the small area known as the American Desert would be equally great, even if it were proved that the ingredients of the Loess were ready there for these winds to act upon. Again, I must urge what seems to have escaped Baron Richthofen, that it is quite clear from every consideration that the Loess belongs to the same geological horizon as the diluvium of the French and

Russian writers. The contents, animal and vegetable, and the remains of man which it contains, as well as its stratigraphical position, all prove this. Again, although the Loess differs from the loams and brick-earths in its texture, and in the abundance of carbonates which it contains, yet its mode of distribution, as we have shown, is essentially the same, both being spread over high ground and low, irrespective of the drainage, and being otherwise similarly distributed. Any theory that accounts for the distribution of the Loess must also therefore explain the idiosyncrasies of the loamy deposits. Assuredly the wind theory of Baron Richthofen would even by himself be deemed incompetent to explain the difficulties of the brick-earths and upland loams of Western Europe. Prof. Aughey urges an objection which is closely connected with this. He says, very properly, that "a fact often overlooked is the transition character of some beds of sand as they shade into the Loess. As beds of Loess and stratified sands at the bottom of Loess sections often alternate, and even sometimes with strata of clay, it is not easily conceivable how subaqueous agency should have formed the one, and Æolian agency the other " (*op. cit.*, p. 280). Lastly, Baron Richthofen argues as if all the débris found in the Loess were subaerial. This is not so, however. In America, in China, and in Europe it is quite true that the land shells prevail largely over the water shells ; but the latter are certainly found in appreciable numbers, especially in the Loess of Iowa, showing that considerable lakes or rivers must have existed, which is again proved by the traces of stratification in certain areas. I think the American geologists exaggerate the extent of these lakes from overlooking the fact that the Loess has been largely transported and rearranged, and thus its contents have been swept over a wide area remote from their original site ; but it is nevertheless absolutely clear that evidence of such former lakes exists, and such lakes in fact exist still.

In Clay, Filmore, York, and other counties, says Prof. Aughey, " there are considerable numbers of ponds, covering from a few acres to half a section of land, grown up around the border with reeds and coarse grasses and sedges, and, where the water is deeper, with arrow leaves, pond-lilies, and other water plants. In every instance where I had an oppor-

tunity to examine them, there was a thin bed of clayey matter mixed with organic materials, from a few inches to a foot or more in thickness, lying on the bottom, and on the top of the Loess deposit. This clayey matter was probably deposited there before the waters finally retired from the old lake bed in which this soil originated" (*ibid.*, pp. 270, 271).

How is it possible to account for such sheets of water under the arid conditions absolutely required by Baron Richthofen's fierce winds and steppe climate? The fact is, whichever way we approach the problem, it seems to me that Baron Richthofen's theory not only fails to explain the facts, but is completely at issue with them. His great name may give the theory a certain ephemeral importance, but it will not bear the test of close criticism when we leave the realms of general hypotheses, and come down to the grim, awkward, tyrannical region of facts.

M. de Lapparent, who is by no means indisposed to give more than due weight to the various evidences of denudation, says of this theory: " Cette manière de voir, dite théorie éolienne, est contestée par beaucoup d'observateurs, pour qui le dépôt de loess est par excellence un phénomène aquatique. *Du moins l'origine éolienne, admissible pour quelques dépôts exceptionnels, ne semble-t-elle pas devoir être acceptée pour les limons qui s'étendent sur de grandes surfaces* " (*op. cit.*, p. 140).

Having analysed the capacity of wind when unloaded and when acting as a mere porter, and tried to gauge what it is able to do in this respect, let us now turn to it when armed with other weapons, such as sand and pebbles.

While the wind when unloaded, and merely blowing lustily, seems incapable of disintegrating surfaces and of the various effects of erosion, and is only a porter of loose materials, it no doubt can, and does, when armed and loaded with suitable tools in the shape of sharp-angled sand or gravel, act as a most efficient file and plane, and it becomes a very excellent modeller of rock surfaces, sometimes polishing, sometimes striating and grooving, and sometimes wearing them down.

Prof. Bonney has stated the case very clearly. He says " the gravel and sand when moved by the wind not only impinge one upon another, but often also are dashed against projecting rocks. These too suffer from the incessant cannon-

ade of those tiny projectiles. Nature made use of the 'sand blast' long before man thought of availing himself of it for drilling and for engraving. When a retaining wall is built by the seaside in the path of drifting sand, a few years suffice to smooth the roughened surfaces of the hewn stones. Loose blocks and projecting craglets on a sandy shore undergo the same treatment. For instance, near Burntisland, in Fifeshire, little knolls of basalt crop out from the sand. This, as it has drifted before the wind, has smoothed and even polished the hard rock. . . . Wherever sand drifts over rocks there the surfaces are worn. Sometimes, as in cases from African deserts, the exterior of some of the harder rocks is so completely polished that it appears as if artificially glazed; in others, when the material is soft, no inconsiderable masses are actually removed by the friction of the drifting sand. Projecting crags are worn into the strangest forms; recesses in the faces of cliffs are deepened, possibly sometimes even excavated; pinnacles of rock are undercut till at last they topple over, as a tree when it is felled. In every desert region in all four quarters of the globe, and in Australia no less, this process is going on. In Europe it is generally rare, as in the strange forms of the Brimham rocks; but in such arid districts as the Libyan deserts, or the plains of Utah and Wyoming, the results are by no means unimportant, and drifting sand must not be excluded from the possible agents of earth sculpture " (*The Story of our Planet,* pp. 93-95).

I have already quoted other examples of the same process from Prof. Hughes (*ante,* p. 174).

De Lapparent says that in the Sahara the limestones are polished like marble, so that the camels have a difficulty in walking. In consequence of the action of blown sand, pebbles are rounded by being driven along, while hard materials, like glass in the windows of the houses in Sylt, are rendered opaque by the erosive action of the sand. In California granite and quartz are polished and striated by the same cause, and the American geologists, Blake, Newberry and G. K. Gilbert, have described remarkable examples of its effects both from California and Colorado. At the confluence of the Colorado and the Virgin rivers Gilbert describes

a hard gravel, in which many of the pebbles have been worn down and faceted by the sand-carrying wind, and hard stones, like quartzites and calcedonies, have been rounded and polished by the same cause; rocks, like basalt and trachyte, have their surfaces more irregularly worn, the harder and tougher crystals projecting, while the limestones are grooved into channels of the most fantastic curves and arabesques. M. Rolland has found pebbles on the Sahara whose surface has been worn into a kind of lace work by the sand. Schweinfurth describes even larger erosions, such as those causing the perched rocks. He describes a natural obelisk more than ten metres high in the form of a reversed pear formed of a mass of perched granite, whose lower parts have been worn off by the blowing sand. Similar phenomena occur at Paria Creek and Rocker Creek in America (De Lapparent, *Traité de Géologie*, pp. 137-139).

In this work the wind simply supplies the motive power. It is the sand and pebbles that furnish the biting teeth which do the actual eroding work, and they do it in proportion to the force and pressure of the wind, just as emery paper or a file would do it in proportion to the pressure applied. The conditions for such action, however, are very local. Most of the earth's surface is protected from denudation by its vegetable covering. It is only when bare rock surfaces are available that it can act in this way, as in the Sahara, in the great stony deserts of South Africa, of Arabia, and of Mongolia, where the air is dry and the materials exist; and to be at all effective the wind must be blowing hard. Under such conditions, and especially when it is blowing through narrow gullies, as in many places in Africa, erosion and denudation will however take place as it does in a glass-etcher's workshop, where the possibilities of the process are daily being tested experimentally.

The very local character of the possibilities and of the results of wind erosion as a geological agency, however, are patent enough. They have given a veneer and polish and a striated appearance to certain exposed rocks, and have caused some fantastic tors to mark certain landscapes, but they have had no real part in sculpturing the main surface features of the earth.

CHAPTER VI.

RAIN AND RIVERS AS DENUDING AND DEPOSITING AGENTS.

" Mr. Ramsay's notions about rivers and river-valleys will probably cause astonishment to the disciples of Mr. Hopkins. Starting with the undeniable postulate that river-valleys are not *necessarily* connected with fracture, he goes on tacitly assuming that rivers always make their own channels. The style of demonstration is peculiar. Referring to the vale of Reading, he asks, ' How did the Thames find its way through what was once that great scarped barrier of chalk now called the Chiltern Hills?' The answer is grand: ' Such phenomena are not confined to this river alone, it is a trick that rivers have ' " (*Geol. Mag.*, ii., 62 and 63).

In the previous chapter I have considered the various agencies which chemically and otherwise dissolve away rocks, and have thus caused a certain amount of erosion, and also the erosive effects of winds. We will now turn to the more important agency of water in motion, in its various forms, and we will first consider it apart altogether from its carrying denuding agencies and tools, like sand and gravel, in its grasp. Water when thus unloaded is, when in motion, an active dynamical agent, but it acts almost entirely as a porter and not as an excavator of solid materials. When unloaded with either gravel or stones it is quite able to carry loads and to transport materials from one place to another, and its capacity for doing so is only to be measured by the amount and the speed of the water actually in motion. This is, in fact, the mainstay of the position for which I have long fought, and to which I shall return again presently. Water unencumbered by solid materials is, as I have said, an excellent porter and carrier, and if it meets with resistance in matter which is movable it will move it if the weight and inertia of the matter in question are not too great for its own pushing power.

This is, of course, a simple mechanical necessity; and the effectiveness of the agent and the amount of work it can do

have been subjected to elaborate calculation by Hopkins and others. The general conclusion of Hopkins, which is unassailable, since it is a mathematical certainty, is that the capacity of water as a transporting agent increases as the sixth power of the velocity of the current, thus the motive power of the current is increased sixty-four times by the doubling of the velocity, 729 times by trebling, and 4,096 times by quadrupling it. Prestwich illustrates this by a concrete example. A spherical block of five tons might be moved by a current of ten miles an hour; a current of fifteen miles an hour would move a block of thirty-six tons; and a current of twenty miles blocks of 320 tons and upwards (*Geology*, i., p. 84). This is a capital measure of the enormous capacity of water to do mechanical work as a porter when its volume and speed are sufficiently great. It is a notable fact which has not always been remembered by geologists, and to which I shall return again in a later chapter.

In regard to the removal of earth and stones by water we must remember another elementary fact, well stated by Sir A. Geikie, namely, that "the average specific gravity of the stones in a river ranges between two and three times that of pure fresh water"; hence these stones lose from a half to a third of their weight in air when borne along by a river. Huge blocks, which could not be moved by the same amount of energy applied to them on dry ground, are swept along with ease when they have found their way into a strong river current. The shape of the fragments greatly affects their portability when they are too large and heavy to be carried in mechanical suspension. Rounded stones are, of course, most easily moved; flat and angular stones are moved with comparative difficulty. Mr. David Stevenson, in his work on *Canal and River Engineering*, p. 375, gives an interesting table of the transporting power of currents of water of different velocities:—

Inches per second.	Miles per hour.	
8	0·170	will just begin to work on fine clay.
6	0·346	„ lift fine sand.
8	0·4545	„ lift sand as coarse as linseed.
12	0·6819	„ sweep along fine gravel.
24	1·3638	„ roll along pebbles one inch in diameter.
36	2·045	„ roll along slippery angular stones of the size of an egg.

M. de Lapparent publishes a similar table, thus :—

Speed per second at the bottom of the stream.		Mean size of the particles.
0·15 m.	clay.	0·0004
0·20 m.	fine sand.	0·0007
0·30 m.	river sand.	0·0017
0·70 m.	small gravel.	0·009

With a speed of one metre pebbles of the size of an egg will be moved, while a movement of 1·80 m. will move flat stones.

M. de Lapparent points out, what it is important to remember, that the speed of the lower layers of a river is much less than at its surface, and that a considerable discount must always be made from any calculations dependent on surface flow (*op. cit.*, pp. 204, 205).

Let us now apply these facts to our problem.

A shower of heavy rain upon a soft surface rapidly pits it with rain marks, and presently with larger hollows ; and few sights are more inspiring to a young geologist than to see these rain marks preserved for all time in the older rocks by having been covered in with dry dust and then consolidated before they were obliterated.

When the rain is heavy, as it is in the tropics, or when it is frozen and falls in the shape of hail, it beats and batters the ground with a considerable force, which is intensified by the reiteration of the small knocks, and thus, no doubt, where the cohesion of certain exposed beds is not very great, they are more or less disintegrated ; while, in regard to hail in tropical countries, where the pieces of ice which fall in that form are very considerable, they cause undoubted destruction in the limited localities where they fall and where the ground is soft enough and is not protected by forest growths or other verdure.

When this process has been prolonged through many decades of centuries, the denudation under favourable conditions becomes no doubt very remarkable, and Lyell long ago pointed the moral of his tale by certain well-known examples, such as those of Botzen in the Tyrol. Bonney, describing the latter, says : " Valleys already excavated in the red felstone (commonly called porphyry) have been partially filled up with a tenacious clay, which contains

many pieces of rock, large and small. A glen has been cut by a mountain stream through the clay into the rock below, and on either side it is fringed by the earth pillars. . . . Each is usually capped by a block of rock like a turban ; some, however, are bareheaded. On this block the existence of the earth pillar depends ; those which have lost their caps, lose not their heads only but their bodies. Here and there the clay slope is furrowed by a rill, but for the most part the nullahs between the ridges and the gaps between the pillars are perfectly dry in fine weather." Bonney adds : " It becomes clear to a geologist after a little scrambling among the pillars that rain has cut the gulleys and even furrowed the sides of the pillars. . . . For a long time the pillars are protected from serious harm by the capstone, as by an umbrella. Still it is very slowly attenuated ; the capstone becomes less and less firmly supported, till at last it slips or is blown off. Then the days of the pillar are numbered ; from a pinnacle it becomes a hump, and at last is wholly washed away." These earth pillars are due solely to the mechanical action of water, for upon clay of this kind it has no chemical effects of any importance. In the Alps they are seldom more than eight or nine yards high, and often rather less, but in America they reach a larger size. On the flanks of Mount Shasta they are gigantic. Mr. Clarence King speaks of " a family of pillars from one to 700 feet high, each capped with some hard lava boulder, which had protected the soft débris beneath from weathering ". But they may be seen also in miniature. " Where there is a bank of clay containing some flattish chips of stone—such as may be found not seldom in North Wales—a careful search is almost sure to discover some tiny models of earth pillars, which perhaps may be as much as a couple of inches high, with capstones rather over half an inch in diameter " (*The Story of our Planet*, pp. 110-113). Bonney further quotes several interesting examples of these earth pillars in Europe as near Stalden in the Vispthal, near Euseigne in the Eringerthal, near the path from Viesch to the Eggischhorn, near Ferden in the Lötschenthal, on the north side of the Brenner Pass, near Molines in Dauphiné (one about seventy feet high), near Sachas in the same district, the last also sixty to seventy feet high, these

being exceptional from the absence of capstones (*ibid.*, p. 110, note).

Prestwich also refers to these earth pillars with his usual graphic force. He says : " Amongst the profusion of illustration it is difficult to choose. Possibly nothing presents a more vivid idea of this quiet and steady rain-work than the grand monumental columns of many parts of the great West. Similar cases are well known in Europe, but they are on a comparatively small scale. The stone-capped columns of Botzen have often been described, and Lecoq figures a good instance at Boudes in Auvergne. But these, although very striking, sink into insignificance compared to those of Monument Park and other districts of Western North America. Amongst the most remarkable of these weathered rocks are those described by Dr. Hayden in the Sawatch district. For about three miles along the side of the south river, and for half a mile in breadth, the wooded slopes are studded with hundreds of these monuments, some of which rise to the height of 400 feet, the average being from sixty to eighty feet high. Spruce trees of great size seem like dwarfs by the side of these mighty columns, each of which is capped by a projecting boulder of very various sizes. In this case, the weathering results from the degradation of a soft conglomerate composed of volcanic sand with trachytic boulders of various sizes. The surface waters and rain flowing over the escarpment of the valley are stayed by the blocks, and then running down on either side of them remove the soft cementing mass, but leave that which immediately underlies the boulders standing as columns, until after a time the boulder itself topples over and the column yields to further pluvial action. Storms assist by beating against their sides and carrying away the smaller particles and the sand " (*Geology*, i., pp. 153, 154).

These are instances—and notable instances—of what we all admit without any question, namely, the capacity of rain to remove soft materials and to carry them elsewhere, and thus in certain limited and favourable localities to act as a considerable denuder.

It is not only loose materials, however, that water in this form can erode. When the impact is considerable and the blows are repeated, even drops of water may cause erosions

in fixed and harder materials. We are familiar with "the pitted, channelled surface of the ground lying immediately under the drip of the eaves of a house," and with the similar effects on the pavement underneath railway bridges, in dripping caverns, etc.

The same kind of effect on a larger scale is, of course, the result of water when *en masse* it either falls perpendicularly from a considerable height, as in various spouts or waterfalls, or is thrown against masses of rock by the force of a rapid river. In these cases there is a continuous impact and hammering effect, and the result is the erosion and scooping out of a hollow in the rock where the impact takes place. Examples of this are too common for reference, a very notable example being the pool at the foot of the Niagara Falls, and similar pools at the foot of other waterfalls. It is the same with the rapid and sudden impact of large masses of water like the Aar, when rapidly traversing its famous gorges at Meiringen, or the river which drains the lower glacier at Grindelwald, or the Reichenbach Fall. When coming down their rapid beds these great masses of water are thrown with a violent shock first against one side and then against the other of the gorges through which they pass, and they have thus hollowed out and smoothed the surfaces on which the water has struck. The same result takes place on rocky coasts exposed to the full bang of the tide, and where the rocks are of uneven texture. These results, however, are very local indeed, and are not what is meant by the geologists who appeal to erosion by water on a great scale. If we were to measure all such effects and cumulate them, they would amount only to a very slight alteration of very limited local surfaces.

Let us turn, therefore, from the denuding work of water acting by repeated impact to its more normal mechanical work, as observed in rivers and the sea.

As we have seen, Mr. Beete Jukes unhesitatingly attributed the greater part of the shaping of the earth's contour to the action of denuding agencies, especially rivers, and he virtually assigned to them the manufacture of all, or nearly all, our valleys. This view is very largely held by living geologists, and notably by my friend Prof. Bonney, whose moderation

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and judgment make him such a trusted guide in our science. " Valleys," he says, " from one end to the other, bear testimony to the erosive action of water. Formerly they were generally regarded as fissures produced by movements of the earth's crust ; . . . but now it is generally admitted that while these and other consequences of earth movements may have produced indirect effects on the course of streams, the rivers practically made the valleys, not the valleys the rivers " (*The Story of our Planet*, p. 116). To this view I take complete exception. I have written against it for a great many years. It has always seemed to me to involve one of the most retrograde steps ever taken in the philosophy of geology, and I will proceed to give some reasons for my view. First, I must refer to a parenthetical postulate of my position.

In facing a problem like the one before us it is well to realise what we really want to explain, and we must carefully discriminate between two issues. Is our purpose to explain and describe the surface contour of the earth as we see it in hill and vale, or to describe its solid skeleton ? It cannot be too emphatically urged that the two problems are very distinct.

In the eyes of a certain number of geological students the contour of the earth's surface is measured almost entirely by its mere surface configuration. The shape of a valley to them is the curved or angular line taken by its surface beds. What they try to explain, in explaining the origin of hill and valley, is the origin of hill and valley as they would be marked on a contour map of the trigonometrical survey. This is, of course, most misleading if our object is to analyse not the skin but the bones of our mother earth. The real outline and structure of the earth's skeleton are disguised and not disclosed by the soft mantle of clay, sand and loam which covers it, just as the flesh of our bodies disguises the contour and materials of our own skeletons. It is almost as easy to realise what the actual contour of the solid floor of a wide river valley like that of the Thames is from looking at its green fields and meadows, as it would be to describe the shapes and positions of our many bones, if we could only judge of them by the external surface of our bodies, and had no actual skeletons to judge

them by. This has not been sufficiently recognised in geological manuals; and many students argue as if when we have stripped off the gravels and loams which are spread over the country with such smooth and gentle outlines we shall find underneath a correspondingly smooth and equable contour, and that the explanation of the one is also an explanatory key of the other. It is only after inspecting sections of coal-mine shafts, or of deep railway cuttings, and such works as the great Manchester Ship Canal, that it is seen how scoured and torn and ragged *the surfaces* of the solid strata are, and how broken by faults and fissures where the superficial contour is so smooth. When this has firmly fixed itself on one's mind, it brings with it the conviction that whatever shaped the solid skeleton of the earth, it was something very different to that which covered over the wounds with their soft mantles.

We can see at once, when our attention is fixed upon the subject, that the tearing or lifting or tossing about of the hard tough rocks, which form the real crust of the earth, and the moulding of them into their present contour and form, is a very different matter to covering over the whole with more or less continuous sheets of soft and easily transported materials. It is as different as the process of building a wall is from that of covering its ragged features with a layer of plaster or paint. One of these processes may require a great exercise of strong coercive or destructive force, the other may be merely that of a transporting agent which has the power of laying down a coat or blanket of soft material or of taking it up and carrying it. One may fitly be described as erosion and the other as porterage. Those who write on denudation are greatly apt to confuse these two processes. The distinction has in fact been strongly insisted upon by my friend Prof. Bonney.

Let us now turn to the concrete problem before us. Every river has really a double life and does a double dynamical duty. When its course is rapid and its flow irregular it washes away its banks, and sometimes its bed, and is an active transporter of loose materials, but this is only in its upper career. Presently every river becomes sluggish and slow, and therefore incapable of carrying materials of the

same specific gravity which it carried in its more youthful and boisterous upper waters. Instead of being an active porter, under these conditions a river entirely reverses its former functions, and proceeds to lay down what it before carried along. As it slackens its pace, and sometimes reverses its own current slightly, it proceeds to lay down the heavier materials it has brought with it, and to lay them down in its own channel, and instead of being a scouring it becomes a depositing agent. It proceeds, in fact, to raise its own bed instead of eroding and cutting it out.

The next thing to remember, as a slight corollary from the statements just made, is that a river has also, when treated as an excavator, two different functions, and we must separate its function as an excavator and eroder of hard materials and of rocks *in situ* from its function as a porter of soft materials, which it simply takes up and carries away. The former function it performs chiefly, or, in fact, I may say entirely, when it is loaded with sharp weapons and tools in the shape of gravel, sand, etc. The latter it can perform when unloaded and unarmed. We will first try and analyse the latter process.

It is easily seen when rain is splashing a sandy or loose soil that the grains of sand are displaced, and that as the raindrops collect into rills these rills become small porters of the loose material, and as they collect into larger streams they become capable of greater efforts in this respect. The process by which a stream is thus formed by mere porterage has been graphically described by Beete Jukes. " If," he says, " we watch the tide receding from a flat muddy coast we see that the mud-flat, even when no fresh water drains over it from the land, is frequently traversed by a number of little branching systems of channels, opening one into the other, and tending to one general embouchure on the margin of the mud-flat at low watermark. The surface of the mud is not a geometrical plane, but slightly undulating ; and the sea as it recedes carries off some of the lighter and looser surface matter from some parts, thus making additional hollows, and forming and giving direction to currents, which acquire more and more force and are drawn into narrower limits as the water falls. Deeper channels are thus eroded and canals supplied for the drainage of the whole surface. First two

and then more of these little systems of drainage unite, until at dead low water we often have the miniature representation of the river system of a great continent (wanting, of course, the mountain chains) produced before our eyes in the course of a single tide."

Let us analyse the process here described somewhat. In the first place, it is entirely a process of porterage rather than of denudation, the particles of sand are moved and pushed on because they are loose. In the next place, the whole process is not due to any fantastic capers of the water—to what the Americans in their graphic slang phrase would call the *cussedness* of the water—but to its anxiety to run away in exact accordance with the laws of gravity, and in order to do so it has to make itself a channel of least resistance along which to flow. Directly it has attained this end its work as an eroder ceases, and it marches along its route quite peaceably, and will do so for all time without disturbing the ground on condition that its volume remains the same. If its volume increases it must enlarge its channel to accommodate it, of course.

In the process of finding itself a channel where it meets with least resistance, it in places impinges on its banks either on one side or the other, and as these banks are confessedly soft and yielding, it undermines and carries them away somewhat, and thus acquires a meandering course. And when the meanders have started they have a tendency to increase, since the flow of the stream makes it impinge in such a case first on one bank then on the other.

This process has been described by my friend Prof. Bonney with his usual graphic force. He says: "One of the most characteristic features of streams, whether large or small, is the tendency to wind in serpentine curves when the angle of declivity is low and the general surface of the country tolerably level. This peculiarity may be observed in every stream which traverses a flat alluvial plain. Some slight weakness in one of its banks enables the current to cut away a portion of the bank at that point. By degrees a concavity is formed, whence the water is deflected to the opposite side, and so bending alternately from one side to the other the stream is led to describe a most sinuous course across the plain. By

this process, however, while the course is greatly lengthened, the velocity of the current proportionately diminishes until it may, before quitting the plain, become a lazy creeping stream which, in England, is bordered perhaps with sedges and willows."

Mr. Deeley again describes this action of a river lucidly and neatly, and I am tempted to quote his description. He says : " The stream rebounds from side to side at every turn, and constantly tends to accentuate the sinuousness of its course by cutting its banks. The river however does not become broader, for the material washed from the outside of a curve, or brought down by tributary streams, is piled upon the inside of the curve, and the channel as it were moves bodily sideways. The filling up of the shallows on the inside of every curve with gravel, mud and brick earth, results from the fact that the centrifugal force of the water at the surface as it sweeps round a curve is greater than that of the slower current at the river bottom. The flow is consequently diagonally across the bottom, and carries stones, etc., with it much as the leaves in a saucer of tea are piled up in a central heap when the liquid is stirred. In fact the mass of the water rotates as well as moves forward, sometimes in one direction and sometimes in another, according as it is deflected by the curves of the river. At such bends the stream is deep and narrow, but in the straight reaches this rotary motion does not occur, and the river shallows and broadens. Gravel and sand are not thrown up as a rule on the inside of a curve to a height much exceeding the ordinary water level of the river. On the top of the coarser material pure sand and mud then collect, mainly at flood times, between grass, reeds and rushes where they root in the shallows, and in this manner the level is raised to flood line. It thus comes about that the river deposits a bed of alluvium, the lower gravelly portion of which has a thickness equal to that of the river in ordinary flood, while the upper surface of the sand and brick earth on the top indicates the extreme flood rise. In the course of time the sinuosity of the curves becomes so pronounced that the bends approach each other ; the river then cuts through the narrow peninsula and deserts a portion of its course, which slowly silts up. . . . The active nature of this looping tendency may be seen by

examining any good map of a district where two counties are separated by a river, for in such cases we shall find patches of a county stranded on the opposite sides of the stream. As long as the river moves bodily in a lateral direction it of course carries the boundary line with it, but when a loop is cut off an island is first formed and then by the silting up of the old channel through which the boundary passes a piece of land which stood on the right bank is transferred to the left bank or *vice versa* " (*Geol. Mag.*, 1897, pp. 389-390). I have the more satisfaction in quoting this paragraph because I differ absolutely from almost every other statement in the article in which it is contained.

The process here described, as I have said, is one of porterage, and involves the cutting out of a channel in soft easily movable material and its transport elsewhere ; and a very large proportion of the materials transported by rivers is not in any way the direct product of erosion upon the solid foundations of the valleys, but merely the transfer of a certain mass of loose materials from the banks and bluffs which line the currents to some other part of their course. The amount thus transported is no doubt sometimes stupendous. I will mention only one calculation made by Messrs. Humphreys and Abbot in regard to the Mississippi. They conclude that the average amount of sediment contained in the water of the Mississippi is $\frac{1}{300}$ by weight and $\frac{1}{2000}$ by volume of the water, and that 815,500,000 pounds of mud are annually carried by that river in suspension into the Gulf of Mexico. This may or may not be a very exaggerated estimate. Whatever the amount, it must be very large. But a very unscientific and ridiculous use has been made of the fact.

The problem is generally treated in a most perfunctory manner. The water at the mouth of a river is analysed ; the proportion of solid matter in it is tested and sifted out. The whole quantity being carried by the river is then calculated, and finally we are pointed to the result as an irrefragable proof of the vast erosive powers of a river.

The initial fact is quite true, but we must always remember that the materials thus moved and carried along are for the most part already loose and movable. They are only a part of the loose covering of the earth, and if we measure and weigh

them we shall have no measure whatever of the eroding or excavating power of water as applied to the hard and solid stratum of the earth. Even when thus limited this movement does not always mean the same thing. When the stream or river is first formed, and has to carve out a road for itself along which to travel, it no doubt does, when running through a mass of soft deposits, carry away the materials which lie below it and which impede its easy flow, and it continues to do this until it has shaped for itself a channel of least resistance, a more or less frictionless path. When this has been secured, except when the current is rapid and boisterous, or when replenished by some abnormal flood, there is reason to believe that a river ceases to deepen its channel.

The flow of a river, however, as we have seen, is not always sufficiently gentle and definite to prevent it from making inroads sometimes upon its banks, and generally the inroad is more or less persistent against one bank. In rivers flowing north and south, or in the reverse direction, this has been supposed to be due in some measure to the effect of the earth's rotation on its axis. It thus comes about, as is familiar enough, that under certain local conditions a river flows in a channel within a channel. The smaller channel—its real bed for the time being—is what the French term its *cours mineur*, while the outer and bigger channel is what they call its *cours majeur*. The latter marks the extent to which the river has at different times eaten away its banks at the place in question. This is a familiar enough incident in the lives of rivers, and it has been employed by some geologists to support the notion that all rivers are thus engaged everywhere in cutting out and displacing the soft materials in the valleys in which they flow by a process of gradual eating it away, first in one direction and then in the other. Here, again, we have an example of the pernicious habit of making small and local effects do service in an argument for necessities imposed by very big problems. The fact is that this corroding of banks is not an unlimited, but quite a limited and local and casual proceeding, and in much the larger part of the courses of rivers no such active corrosion is taking place at all, but there is a most conservative and steady adhesion to their primitive beds by the rivers.

I had worked out this view when I met with the following sentence of M. de Lapparent's, which I completely endorse. Speaking of the inferences drawn from the amount of matter in suspension carried down by certain rivers, he says : " On a cherché quelquefois à édifier, sur des données de ce genre, des calculs ayant pour but de déterminer que doit perdre chaque année le sol d'une contrée pour fournir les éléments solides transportés par les rivières qui l'arrosent. C'est ainsi qu'on trouvera, dans plusieurs ouvrages de géologie, l'indication du nombre de millions d'années qui devrait suffire pour la disparition totale d'un continent, à raison de tant de milli-mètres ou de fractions de millimètres par an. Nous ne nous arrêtons pas ici à ces spéculations, dont le tort grave est d'étendre à une suite d'années presque indéfinie des données qui s'appliquent tout juste à l'époque où nous sommes. D'ailleurs, loin de s'accomplir sur toute la surface d'un bassin, l'œuvre de l'érosion se concentre dans des régions très limitées, et puisqu'il est avéré qu'elle tend partout vers l'état d'équilibre, on peut admettre qu'après avoir affouillé avec une certaine énergie les parties les plus voisines de leur lit, les rivières doivent devenir peu à peu des agents d'érosion de moins en moins efficaces, à moins qu'un changement ne sur-vienne dans les conditions de leurs pentes. C'est ce qui rend à nos yeux, tout à fait illusoires les calculs dans nous venons de faire mention " (*op. cit.*, p. 218).

Apart from this sound *a priori* reasoning we have innumer-able tests in the various monuments which line river banks, towns, houses, bridges, fords, piles, jetties, trees, hedges, etc., which stand where they have stood for generations, and which clearly prove that the rivers where they are found have not been the vagabond agencies which they are made by some geologists in search of a cause to sustain their hypotheses, but have been for the most part content to follow the same route which they followed in the days of their youth.

I will quote a very few cases illustrating what I mean, and will begin with one or two facts of an historical character.

There is hardly a ford or an ancient bridge in any part of the country which does not afford evidence that the rivers are virtually flowing at the same level as

they have flowed for many centuries. Such instances as the Cowey Stakes at Kingston, the foundations of Old London Bridge, the position of the marble quays on the Tiber, in fact the evidence of nearly every riparian town known to me speaks the same lesson, *viz.*, that the great majority of rivers having secured a comfortable bed lie in it without disturbing their banks. Elie de Beaumont has enlarged on this side of the issue.

He quotes the remains of several Roman towns to show how little, if any, has been the erosion of rivers since they were built. For instance, the Roman constructions in Strasburg are so low in regard to the river Rhine, that unless we suppose the former inhabitants built their town below the level of the Rhine there cannot have been any change in the relative position of the river. The same is the case at Lyons (*Leçons*, etc., pp. 142, 143). Near Soleure in Switzerland Roman buildings are found very little above the level of the Aar. "Ce qui prouve," says our author, "clairement que cette rivière ne s'est point abaissée." Many Roman bridges still exist, and we do not find the river water leaving them high and dry or apparently rising any higher than it did when the bridges were built. M. de Gasparin has published some interesting observations in this behalf in regard to the Rhône. At Arles the waterpipes of lead which carried the water to the town still lie on the bottom of the river. The ditches of the castle of Tarascon, cut in the rock in very early times, are periodically filled by the Rhône water, and do not remain permanently full as they would if the river's bed had sunk. The piles of the bridge of St. Binezet at Avignon remain relatively as they were. The bridge of the St. Esprit, built like the latter in the sixteenth century, remains in the same position in regard to the river as it did when built. The piles of the Roman bridge at Vienne are still planted deep in the bed of the river. Such facts as these go to show that the river has not altered its level since very early times (Elie de Beaumont, *Leçons*, etc., pp. 143, 144).

If we turn from historical to geological facts the same conclusion is inevitable. It seems to me that we can hardly have a better proof of the slight effects, if any, that rivers have in eroding valleys after they have secured an even bed

to flow in, and where their flow is not rapid and torrential, than the great carpet of humus which not only covers every valley bottom, but is actually growing and accumulating. The same conclusion follows if we examine the deep and still growing beds of peat in many river valleys, like that of the Somme, for instance, where the bottom layers contain large numbers of remains of the stone age, showing that from that time there has been a growth of the soft materials in the valley and not a destruction of them.

Again, a large number of rivers, especially sluggish rivers, pass through lakes in some part of their course, and in some cases through a series of lakes. If rivers had been engaged in the porterage of the vast masses of material involved in the orthodox theories of pluvial erosion, these lakes ought, as Sedgwick remarked long ago, to have been gorged with such materials and been filled up.

Again, " the eskers covering the valley of the Shannon, the ' Rock of Killaloe ' and the limestones at Doonars," says Mr. Kinahan, " show what small power a river has to deepen its bed. This mighty river has been running over these gravel ridges and these rocks ever since the esker sea period, yet it could not cut to the base of the eskers ; while at Killaloe it was only able to remove the drift off the rocks, and similarly at Doonars the limestones are but slightly worn, except along the lines of joints " (Kinahan, *Geology of Ireland*, pp. 314-324).

The remark here made by Mr. Kinahan can be widely applied. The lower valley of the Brora in Sutherland is typical of a large number of valleys in the mountain districts of Britain, in which we have left, without apparently the slightest change of outline or contour, the drumlins, eskers, etc., just as they were left by the forces which deposited them, all, too, made of the softest materials which running water might be expected to wash away directly, and yet all intact with their original shape and contour.

Nay, more, there is hardly a river in that part of England which is marked by so-called glacial beds which does not flow in a cushion of boulder clay of gravels and sands dating from the last period of the world's history, and which it does not in the slightest degree disturb or move. If rivers were

capable of denuding valleys of their softer garments, how does this come about?

Prof. Hull (*Geol. Mag.*, iii., p. 117) calls attention to certain valleys which are crossed by water-sheds whence springs flow in opposite directions, that is to say, they contain no rivers running right through them, and he quotes four notable cases, namely, the valleys of Todmorden and Cliviger, Whitworth and Sabden, in Yorkshire and Lancashire. The valley of Todmorden, he says, does not narrow at the watershed, and for some distance on either side of the water-shed the brooks are so insignificant that they cannot be considered as having modified the form of, much less of having been the agents in hollowing out, this deep furrow. Nor is there any sign in the shape of terraces, etc., that the rivers in these valleys formerly flowed in a different course.

This being the direct evidence of the capacity of the present rivers for excavating their beds and banks when composed of loose materials, and the further evidence gathered from historical and geological data as to what they have been doing in this behalf since we have any record, let us now turn, shortly, to the complementary function of rivers in certain parts of their course, namely, when engaged in the very opposite business to erosion, *i.e.*, when depositing materials. As I have already said, a river as a mechanical agent performs two entirely opposite kinds of work upon its own channel. When its fall is great and its flow rapid it acts as a scouring agent, when loaded with stones and rubble and sand it erodes its sides and bed, and when not so loaded it takes up and carries along the soft materials it meets with and which are within its capacity to carry.

As it slackens its pace and loses its early sprightliness it cannot, as we have seen, carry or move the same loads, and it proceeds to deposit them in its bed, roughly, according to their specific gravity, the lightest and most pulverulent materials being carried the furthest. In this way every sluggish river has a tendency not only to raise its own bed but to lay down deltas and bars, the latter of which are nearly always formed of the very finest material; it having been said, for instance, of the delta deposits of the Euphrates and Tigris that they do not contain a stone the size of an egg.

The deltas deposited by some of the larger rivers are very large, and notably those of the Mississippi, the Ganges, the Brahmaputra, the Nile, the Danube, the Rhine, etc., and if their rate of deposition had been constant might be used as valuable chronometers, and I sometimes think their value in this respect has been underrated, in consequence of some unfortunate deductions, like those of Leonhard Horner in regard to the age of pieces of pottery found in the Nile mud.

The delta of the Ganges is 8,000 square miles in extent, and its apex is 220 miles from the sea. That of the Rhine stretches inland for eighty or ninety miles, that of the Brahmaputra, which is the largest, is as big as England and Scotland.

The growth of deltas is naturally rapid. Mohammara, in the delta of the Euphrates, is supposed to mark the site of Charax, which was on the sea in Macedonian times. Arles, which was fourteen miles from the sea in Roman times, is now thirty, etc.

The greater part of these deltas consist of the finest silt, but when we dig down we find in some cases beds of pebbles and gravel. Such beds of pebbles have been found at depths of 175 to 185 feet, 300 to 325 feet, and throughout the lowest, eighty-five feet, in the deep boring of the Ganges delta ; in the Rhône delta at a depth of 300 feet ; in the Rhine delta at 191 feet (Prestwich, *Geology*, i., pp. 88-90). This has been supposed to prove that when they deposited these beds the rivers had a much greater transporting power than they have now. I believe the explanation to be a very different one, and shall enlarge upon it later.

In addition to depositing fine silt in their deltas, there are certain rivers, like the Nile, the Yenissei, etc., which from various causes overflow their banks annually or periodically, and spread layers of what is known as warp over the ground where their waters have been. As a measure of the amount of this deposit, L. Horner pointed out that the base of the statue of Rameses II., which was erected 3,212 years ago, is now nine and a half feet below the surface of the ground.

Let us now turn to a third mode in which rivers increase,

instead of eroding, the deposits in the valleys they flow through.

The deposition by a river of the load it is carrying in its own channel naturally raises it, until presently the river actually flows above the level of the surrounding country, and has to be carefully banked and dyked. However carefully this is done, it sometimes breaks through, as the Lower Rhine has often done, and as the Lower Thames, nearer home, has done, devastating the farm lands in the Essex marshes, and Debenham Breach is a notable monument of its handiwork. When the river has escaped from the aqueduct on which it is travelling, it spreads over the lower ground and proceeds to find itself a new route. Reclus has given an interesting map showing the meandering dykes along whose top the Lower Rhine once travelled.

These solid aqueducts occur in most rivers charged with sediment and flowing sluggishly. Thus at Borgoforte, between Mantua and Modena, the Po has raised its bed since the thirteenth century by more than 5·50 m. (Dausse, *Bulletin de la Société Géologique*, 3rd ser., iii., p. 137 ; De Lapparent, p. 198). The Yellow River is a famous example of the power of a river to raise itself aloft in this fashion, and (presently, when its fragile banks break down) of rushing through them and changing its course enormously, to the extent, indeed, in this case of finding itself a fresh mouth a hundred miles away.

The extent to which the level of valleys has been raised by this depositing capacity of rivers has hardly been realised. Thus, in the Thames valley, Mr. J. E. Howard, F.R.S., says, " the first of the strata at which you arrive in digging the foundations of houses in London, and I have had some personal experience of this recently within a few hundred yards of St. Paul's, consists of sand and gravel, and contains some remains of the Roman period. Then beneath these you arrive at strata, which (I am told) contain the bones of the mammoth and other extinct animals. I do not think we can arrive at the conclusion that there has been since then any excavation, but quite the reverse, when we find these strata superimposed upon each other about twenty or thirty feet under London. . . . We know that the rivers in the

neighbourhood of London do not now excavate the valleys at all; it is rather the contrary, for they appear to fill up very considerably. This I know to be the case in regard to the river Lea, near which I live, and in the neighbourhood of which I have works, and have seen excavations. The Lea valley, in the vicinity of Bow, has been filled up since the Roman period to the extent of five or six feet, as is shown by the excavations that have been made; for the workmen have found, and I have received from them, many interesting and curious relics of Roman times. Therefore I am unable to understand the argument we have heard as to the formation of valleys by slowly flowing rivers such as the Thames. It does not seem to me that in any conceivable time, even if you were to take an eternity, you could excavate the valley of the Thames by means of the river flowing through it; it would rather, as I have already said, have a tendency to fill up the valley."

In his reply to this argument, Prof. Hughes confessed that the Thames, in its lower reaches, is not doing the work of excavation; " for the denudation," he says, " we must go higher up the valley ". A member of the Victoria Institute, writing in reply to this from Cirencester, close to the head-waters of the Thames, stated that he had not observed evidence of the " cutting back " higher up the stream of the Thames—a view in which I completely concur, for I have examined the same ground with some care, and it seems to me to point very closely to the suggestion being an unverified premise only.

It seems to me to be as clear as any inductive position can be that if we examine what actual rivers are doing, and do not go to hypothetical rivers, and if we measure their work not by quite exceptional sections and reaches (where phenomena of erosion no doubt occur), but as a whole, that the rivers of the world are, and must be, depositors quite as much as they are excavators, and that their work as excavators has been limited almost entirely to making themselves a smooth and unimpeded bed in which to lie and in which to flow.

This being the conclusion to be derived not merely from *a priori* reasoning, but from actual examples of rivers which have been flowing for generations in their present courses, it

is very difficult to understand how the views of such able geologists as Prestwich, Hughes and others upon the history of the river valleys, which have been sharply controverted by Andrews, Parker and many more who have examined the problems, in particular cases with critical care, can be sustained.

We ought clearly to realise the extent to which the theory of river erosion has been pressed by its champions when writing in regard to the rivers of Western Europe, for the problem is of the deepest interest, not to the geologist only, but also to those who are trying to discover the origin and early history of man as disclosed in so-called palæolithic remains.

As is well known, every river valley, or almost every one, has, running down its flanking sides at a considerable elevation, one or more terraces or bands of similar gravel, clay and brick earth to that now occupying the valley bottom. These terraces, we are told, mark the successive heights at which the rivers formerly flowed. Lyell and Prestwich do not hesitate to say that the highest of these terraces marks the initial level of the bottom of the valley when the rivers began their work of denudation, and that the whole of the mighty mass of materials which is not now to be found there, and which we are told once filled the valleys up to the height of these highest terraces, has been swept and scoured out by the rivers. This tremendous postulate, which is purely arbitrary and wanting in empirical basis, is thrown at us without any real analysis of the conditions and possibilities of the problem. In the first place, if the valleys were once filled up with diluvium to the level of the higher terraces, we have to explain how the present rivers can have removed it at all, for a great deal of this diluvium consists of very coarse gravels, and it contains great boulders and masses of grey wether sandstone, etc., quite out of the reach of any such rivers to transport or move, even in times of greatest flood. This has been strongly urged by De Lapparent. He says : " The Seine, whose speed in ordinary times is about 0·50 m. per second, only transports fine sand, and all the excavations which have been made in its bed show that it has only deposited sand there." He says, further, " that the ordinary current of a river

can with difficulty be increased in speed threefold, even in the greatest floods; accordingly the greatest speed which the Seine is capable of is only 1·50 m. per second at the surface, *i.e.*, about 0·70 m. at the bottom. So that, in times of greatest flood, it can barely move small gravel. . . . In the greater part of the large rivers the transport of any material bigger than fine gravel is an exceptional thing."

De Lapparent here argues that the diluvium and its deposit cannot be explained by the action of the present rivers. Be it remembered, further, that if the present rivers were flowing at the height of their upper terraces, their flow would be still slower than it is now, and their capacity as porters of gravel and other similar materials would be *pro tanto* diminished.

In regard to this part of the subject I should like to revert to the critical instance of the Somme. In the forty-fifth volume of Silliman's *Journal*, second series, is a valuable paper by Dr. E. Andrews, of Chicago, on the deposits in the Somme valley, etc. Although I cannot agree in the use he makes of ice in explaining these deposits, I am anxious to quote the following paragraph, as stating the case well against those who urge that the river terraces are merely the fringes of a former continuous deposit. He says: " The valley of the Somme is over a mile and a half at the top, while the present river does not appear to exceed fifty feet in breadth. It is safe to say that the present stream spread over the whole valley would not be half an inch deep, and, making all probable allowance for spring floods, it is wholly inadequate to the production of gravel beds containing pebbles larger than a man's head and boulders weighing a ton. This valley presents none of the characteristics of those which are widened by the meandering of a shifting narrow stream, now widening this bank and now that. It is broad, level floored, and parallel banked " (*op. cit.*, p. 184).

Dr. J. C. Southall, in replying to Prof. Hughes, says that " the latter rests the antiquity of quaternary man on the fact that the palæolithic implements of the river gravels antedate the excavation of the river valleys by the present streams, and that the time required by the Somme to excavate its valley is the measure of the age of the upper gravels and the implements found in them; for there are ancient terraces along

the banks of the river, and these terraces mark the former position of the stream. The stream," says Southall, "if it spread at the time over the almost level plateau, must have had a depth of less than an inch. The course of the river above Amiens to its source, eighty miles, is a winding one, which tended still further to weaken the force of the current.

" I do not comprehend how Prof. Hughes deems it possible for such a stream to excavate a valley a mile or a mile and a half wide and 150 to 200 feet deep. If it be true that man witnessed the commencement of such a work of excavation, he is old indeed, the time since his appearance on earth is, in fact, almost incomputable. Prof. Hughes, indeed, points out that there has been no change in the valley in 2,000 years, and we may confidently believe that the present stream will not materially augment the excavation in 20,000 years.

" The upper gravel beds exhibit multitudes of chalk pebbles larger than a man's head, and some few far-travelled boulders of sandstone weighing a ton. The shallow stream we have spoken of (less than an inch in depth), moving by a circuitous course, with a fall of eight inches per mile, is supposed to have swept the chalk out of the valley, to have moved and rolled these pebbles and boulders, and to have laid down gravel beds sometimes twenty feet thick."

Again, Mr. Callard tells us he had been all over the ground and examined it carefully, and he had come to the conclusion· that the Somme river, although running through the Somme valley, never excavated that valley. He says : " There are about twenty-eight miles of the valley between St. Acheul and Moulin Quignon, in both of which places implements occur. St. Acheul is 149 feet above the level of the sea at St. Valéry, and Moulin Quignon 106 feet above the same level If, then, the river ever ran at the height of these gravel beds, the fall would be forty-three feet between these places. A fall of forty-three feet in twenty-eight miles gives a good deal less that two feet in a mile," and he asks : " Is it possible that a river flowing with a fall of less than two feet per mile could have eroded this immense valley ? Again, the Somme is but a small, narrow river, while the valley through which

it flows is wide, being sometimes two or three miles in breadth, and I would venture to say that if you could spread the river all over the valley I could walk across it without having my shoes covered with water. I am sure," he says, " Prof. Hughes will agree with me that there is no erosion going on at the present time, and if that be so the data for calculation are taken away. I may add that I took a boat and rowed for five hours up the river to see whether I could find the continuation of the banks that could have kept the river in, for we know that where there are no banks there can be no river." He says he went with a friend and examined both sides of the river, but could not find the necessary banks. Correctly speaking, there was no bank at all, but simply a rising ground stretching back into the country. From all the appearances he saw, he declares the river never flowed up to the implement terraces. With the contour of the country as it is now the river never could have touched the place where the implements are found (" Remarks on Prof. Hughes' Paper," *Trans. Vict. Inst.*, pp. 15, 16).

In answer to Mr. Callard's principal argument, Prof. Hughes said we had no reason to believe that the valley was ever filled up with water right across. A river is continually changing its channel in the low ground, etc. Here I confess I cannot follow my friend (at whose feet I have so often sat) at all. Rivers, it seems to me, which change their beds frequently on low ground are depositing and not excavating rivers. In regard to the possibility of a river eroding a valley by lateral movements, it seems to me impossible and incredible that a phenomenon which is very local and due to very local causes could operate along a whole river valley; besides which, it implies that this work is a kind of see-saw, and that a river having planed off a section of a valley could turn back and perform the process in the opposite direction, which seems to me contrary to every fact I have learnt about river erosion.

It has been the difficulty of meeting these and similar issues which have made Prestwich, De Lapparent and others discard the present rivers altogether as insufficient and incompetent to do the work, and take refuge in quite transcendental rivers, entirely different to those we know, a process

which seems to me to lead us away from science unless it is amply justified by evidence.

Thus De Lapparent says : " *Lors donc que, soit au fond du lit mineur, soit sur le lit majeur, on rencontre des nappes de gros cailloux, on doit en rapporter la formation à une époque antérieure, où la rivière avait, momentanément peut-être, une pente suffisante pour imprimer au courant une vitesse torrentielle. Or ce fait s'observe dans toutes les vallées, notamment dans celle de la Seine, où le fond du lit mineur est formé par un gravier à gros éléments. Ce dépôt doit donc être considéré comme l'effet d'un régime différent de celui qui prévaut aujourd'hui; sans doute il est l'œuvre de la période pendant laquelle la Seine creusait encore son lit, sous la double influence d'une pente plus forte et d'une plus grande abondance dans les précipitations atmosphériques* " (*op. cit.*, p. 205).

The appeal from present causes to causes unknown to our experience is a very frequent one with professed uniformitarians of the modern school.

In the present case there have been two postulates of this kind. One has been that the rivers themselves were formerly much larger, and the other that the land was once much more elevated, and that consequently the fall of the rivers was much more rapid.

Let us consider each of these postulates. In order that the rivers should have been much larger there must have been one of two conditions. Either the drainage area must have been much bigger, or else the rainfall must have been much greater.

How we are to considerably increase the drainage area of our rivers by any known process I know not. Under any circumstances such an increase would be by one river at the expense of its neighbours, which the facts will not warrant.

On this matter Prestwich says : " An important point to notice in connection with these drifted materials is that they are all derived from strata higher up the same river basin. Thus in the valley of the Medway we find worn fragments of only cretaceous and wealden strata; in the valley of the Thames, fragments derived from the drift, tertiary, cretaceous, oolitic, etc., strata of the London basin; while in the valley of the Severn there occurs a more complex gravel, derived

from rocks of triassic and various palæozoic ages, which are found in that hydrographical area. Thus, therefore, the evidence is clear, that not only have our valleys been formed by river action (! ! ! H. H. H.), but the old rivers were confined to the same basins as at present, and that if the water power was greater (as we shall presently show to have been the case) it was due not to more extensive drainage areas, but to a greater rainfall or greater floods dependent upon climatic conditions different to those which now prevail " (*Geology*, p. 92).

Let us now turn, then, to the second postulate, namely, that of a much greater rainfall in former times. We are told, as I have said, that the rivers of old days were really bigger and contained many times their present quantity of water, and that when thus enlarged the whole state of things would be entirely altered. This is exactly like the wild ice-men, who cannot tolerate references to laboratory experiments, or to nature's own experiments on glaciers, but bid us go beyond the north wind into the wonderland they have created, where all sorts of extraordinary things take place undreamt of by that science known as inductive.

How are we to justify the hypothesis of such transcendental rivers, and when obtained, why are we to conclude that they worked in the method postulated ?

Various causes for such abnormal floods have been assigned. Mr. A. Tylor postulated a pluvial period, characterised by most exceptional rainfall ; others have appealed to the rapid melting of ancient glaciers, while Mr. Belt urged that these floods were caused by the pounding back of the European rivers, and the consequent formation of a European lake by a great Atlantic glacier.

These theories, like those which have been already examined, in claiming the rivers or a lake, either in their normal or abnormal conditions, as the depositors of the terraces, fail to meet some supreme difficulties. If these deposits were the result of successive river floods, or of river floods distributed over a long period, we should assuredly have the deposits arranged in layers, marking the secular variations of the water, such as we find in all river-warp and similar deposits. Here, however, we have no such facts, but the

gravels, sands and loam are deposited according to the laws of gravity, as they would be deposited by one supreme effort. Let us, however, analyse the suggested causes for the Titanic rivers.

Mr. Tylor does not argue that such a rainfall as he requires is possible with the climatic conditions now in vogue, but he boldly accepts the position that climatic conditions were at that time entirely different. He attributes the enormous rainfall of his pluvial period to " the sun's influence, which induced a much greater amount of sunshine in summer and a corresponding diminution in winter " ; but where have we any warrant for such an hypothesis? In regard to the meteorological conditions of the climate when the gravels and river terraces were laid down, we have the best of all barometers ready to our hands in the abundant remains of plants and animals in the tufas at La Celle, etc., which not only mark the mean temperature, but limit also the extremes ; and this barometer, with manifold readings all concurring in one conclusion, is surely conclusive that the climatic conditions of this period were such as prevail now in our temperate latitudes, and quite inconsistent with those required by the hypothesis of Mr. Tylor, and which are necessary if we are to secure such an abnormal rainfall as 300 inches annually in England, which is his estimate.

A fall of rain in temperate latitudes on this gigantic scale is assuredly a contingency verging on the impossible, and especially when we are told that such a rainfall was not a mere exceptional feature, but characterised a whole geological period, to which Mr. Tylor has given the name of the pluvial period.

Mr. Tylor points to the tremendous rainfall that occasionally occurs in the plains of Scinde, to which might be added Brazil, Central America, etc. All this is perfectly true, but in all these cases the meteorological conditions are entirely different. To import "the rainy season" or the torrential rains of the tropics into our temperate climate, and into our part of the earth's surface, is a postulate which demands certainly very rigid proof. That the warm winds heavily charged with moisture moving westward from the mid-Atlantic should be stopped by the high barriers of the Andes, and the similar

warm winds from the Indian Ocean should be similarly stopped by the Himalayas, and in either case should be forced to discharge their contents in vast sheets of rain upon the intervening plains, is not only natural, but is a necessary conclusion. There are no such conditions available here. We cannot suppose that the North Atlantic was ever the nursery of such heavily charged warm winds as the mid-Atlantic, nor have we, with the exception of the Alps, which are not available, impassable mountain barriers in Europe to force these winds to discharge their contents in any way comparable to the rains of the tropics.

By what process are we to multiply ten, twenty or a hundred-fold the rainfall in the west of Europe or the east of America, which must, so far as we can see, always have had a maritime climate. If we modify or divert the Gulf Stream, as it is possible we may have to do when we explain all the facts of the penultimate chapter in the world's history, we shall not increase the rainfall but diminish it ; we shall, in fact, remove our great distiller and the author of our soft and damp west and south-west winds. We cannot in these latitudes postulate vast mountain ranges like the Himalayas, and if we could we have no monsoons and dripping anti-trades to condense ; while the notion that the Sahara was once a sea is now given up, and if it were not it would be quite inadequate to produce such a result. The fact is, whichever way we look at it, the postulate of a pluvial period in Western Europe is a mare's nest, untenable directly we test it as all such hypotheses ought to be tested ; and let us remember what the demands of Mr. Tylor and his friends really have been.

Even if we granted the conditions of the tropics as possible, we are still very far from having secured a sufficient amount of water to fill up the river valleys of Western Europe, in some cases to the brim, and in most to a height of several hundred feet. Mr. Prestwich states the conditions with his usual force. " The greatest flood of the Seine on record," he says, " is that of the year 1658, when it rose to a height of twenty-nine feet. Even in this case a flood of nearly sixty times that magnitude would be required merely to fill the valley to the level of the high-level gravels,

without taking into consideration the more rapid discharge; but neither in this nor in the other cases of modern times are we aware of an increase in the volume of water during floods in these regions to many times the ordinary mean average, whereas we see that in a case such as is presented at Amiens a flood having a volume 500 times that mean would be required to reach the beds of St. Acheul " (*Philosophical Transactions*, 1864, p. 266). But, granting that the meteorological conditions are admissible, which we altogether question, we have still greater difficulties behind. Mr. Tylor urges repeatedly, and it is in fact the burden of his argument, that the enormous rainfall he postulates (many times greater than that prevailing now) would greatly increase the velocity of the streams and rivers, and thus explain the great erosions which, he contends, are witnessed by the deposits in dispute. "Ancient rivers," he says, "may have had many hundred times as much destructive effect on the surface of the earth, for erosive force increases in the fourth power of the velocity, and may have eroded the surface of the earth as much in former times in one year as they now do in 1,000, in cases where rocks were made to slide on clay by excessive supply of percolated water " (*Geol. Mag.*, 1875, decade ii., vol. ii., p. 459). In all this we seem to be on the track of most misleading analogies, and to be led away by a postulate which Mr. Tylor quotes more than once, namely, that the water in a river has a uniform motion. This is true in one sense, but it is very misleading in another. The most elementary knowledge of a fast river obtained in trying to force a boat against the stream is enough to show that, if we take a vertical section of it at any given point, the water in all parts of that section is not flowing with uniform velocity. The current is very rapid in the middle of the river, but dies away as we near the banks, and in many cases is barely perceptible there. The drag of the banks is proverbial, and greatly assists navigation up stream; but the drag of the banks is really the drag of the river bed, and, as a matter of fact, a swift river is a swift current flowing through a cushion of water moving much more slowly. This is a well-accepted fact in hydrodynamics, and has been confirmed by numerous careful experiments. In the case of deep rivers the cushion

probably moves hardly at all, just as the Gulf Stream is a more or less rapid current of warm water flowing in a cushion of much colder and much more quiescent water. It is this property of rivers which in my view probably explains how, in the case of the Neva, the Volga, and other rivers, the freezing takes place first at the bottom, whence detached plates of ice rise to the surface and get united together, forming a natural bridge over a rapidly flowing current. The water in rapid motion would probably never freeze, if that which is much more quiescent near the bottom, which, therefore, freezes much more slowly, did not furnish it with a constant stream of materials for making itself an ice bridge. This has probably been suggested before, although I do not remember to have seen it. It is only mentioned here by the way. The real burden of my argument is, that we must not suppose that, because a river is flowing with a full current in mid-stream, therefore it is denuding its banks and channel in a corresponding fashion. It would seem that large rivers do hardly any work in denuding the land, *except in their head streams.* It is in these that the work of grinding down and of denudation takes place, and the main stream does little more than bear along the mud, etc., in suspension which has been poured into it by its tributaries. All the great rivers known to me are, as regards their main channel, conservative and recuperative, and not destructive.

While such a rainfall as we have been discussing would not in my view increase the denuding powers of the rivers, it ought undoubtedly to have had a very potent effect in scouring the general surface of the land. It would assuredly have washed the high plateaux clean of their mantle of fine loam, and would have swept it away into the rivers, and thence into the North Sea. It might have left the larger blocks of stone there, but the fine loam would certainly have gone; but this is just the reverse of what we in fact find. As a matter of fact, the heavy débris are all in the valleys, while the uplands are mantled and covered with fine loam. Again, such a rainfall would no doubt have carved enormous ravines and rents in the contour of the valleys at rapid intervals, where the immense mass of water falling on the plateaux made itself discharging channels into the main river, thus

breaking the continuity not only of the solid skeleton of the valley, but even more so of its terraces. No such series of ravines and breaks in the terraces is forthcoming however; but the latter run continuously along the flanks of the valleys. Again, how could the lateral dales of the principal valleys be charged with these deposits up to their very heads, as we actually find them, if the flow of the rivers of the predicated pluvial period was so enormous, and at the same time continuously seaward? These would surely have been washed clean. How, again, could the terraces in such a case have had their materials sifted? But Mr. Tylor makes even greater demands upon our credulity. He asserts that during the pluvial period the rate of denudation was 729 times what it is now, equal to the removal of nine inches per annum. Nine inches of solid strata removed every year, when we have only to go to North Wales, or still better, to Scandinavia, to see numerous faces of rock with the fine striæ made in the so-called glacial period still sharp, although exposed to continuous subaerial denudation for unknown centuries, and thence to gauge the real effects of rain and snow as denuding agencies. Even if we could realise such stupendous effects from such causes, and if we could trace whither the débris of all this scouring have gone to, we should still have to face an insuperable difficulty. Mr. Tylor does not deny that during his pluvial period the land was occupied by a teeming life both botanical and zoological, the remains. of which so abound in the deposits we are discussing, but how was this life possible? How, with such a grinding down of the surface, was it possible for plants, either the herbage and grasses upon which the land-shells fed, or the forests. where the mammoth lived, to grow at all? How could the class of trees of which Saporta and others have furnished us. lists have lived under such conditions? How, again, could the animals have done so? Mr. Mellard Reade may well say, again, that no mould could possibly form under these circumstances except, perhaps, in deltas, as it would be removed one hundred times as fast as made. Whichever way we view this theory of a great pluvial period, we are met by incorrigible difficulties and contradictions, and we cannot avoid the conclusion that it is not based on inductive evidence, for it will

not explain the facts, but is rather another example of the danger of the deductive method in science.

Let us now turn to another explanation. Some of those who do not appeal to a pluvial period or to pluvial periods, and yet believe in very much larger rivers once occupying the various valleys, and which are supposed to have ploughed and excavated them, argue that the enormous quantity of water necessary to supply these postulated rivers came from the rapid melting of great ice sheets. What are we to understand by this? We know, of course, that in Siberia and on the barren lands of North America a depth of many feet of snow is melted with extraordinary rapidity in summer, and the tundras which were a fortnight or three weeks before mantled and blanketed with snow are gay with " Alpine " flowers. We know that certain rivers proceeding from tropical mountains which have a heavy winter covering of snow also have their waters gorged during the spring melting ; but this is an entirely different condition of things to that postulated by the ice men. With them the ice age was not an intermittent succession of snowy winters and flowery summers, but a continuous period of dead chilling ice, in which there is no possibility of understanding fierce summer heat sufficient to cause immense and rapid floods, since the whole country was frozen fast. No doubt the sub-glacial streams, which are almost entirely fed from the melting of the surface of modern glaciers, flow more rapidly in summer than in winter and are fuller; but mere glaciers, even if we could postulate them in the necessary districts, could not supply such rivers as would be necessary to fill up the Seine or the Somme valleys or the valley of the Thames. Instead of there being more water in a glacial age, it seems to me there must have been much less. Now all the water that falls tends to gorge the rivers. Then a great part of it would be stored up in ice, and when that ice began to shrink it would not do so by leaps and bounds, unless we are to postulate gigantic jumps in the climate. It would assuredly act like the great Rhône glacier does now. These vast floods caused by rapidly melting glaciers, to which De Beaumont was the first to appeal, seem to be inferences from an entirely impossible state of things in the latitudes of France and

Britain.   In Siberia and North America rivers are hard
frozen in the winter.   When the spring comes their upper
parts are first melted, while the lower ones are held tight by
the ice.   This leads to the ponding back of their upper waters
and, of course, to the gorging of the valleys, and when the
dams eventually break to portentous floods; but this is an
entirely different condition of things to any possible one
which could explain the origin of the river valleys of Western
Europe with their varying and often divergent courses, and
in places and in latitudes where such dams cannot for a
moment be thought of.

Again, the courses of the sub-glacial streams, as postulated
by the believers in ice sheets, are most difficult even to con-
ceive.   How could the greater part of the English valleys
have contained streams at all when the postulated ice sheet
was pushing its great heavy foot athwart their course?   How
could the rivers now running east and west and north in
Northern Europe and America have run at all when the
supposed ice sheets were scraping along, right across or in
the opposite direction to their courses?   Assuredly it is in-
credible.   The stream from the Rhône glacier flows down
the Rhône valley, and does not flow underneath and across
the line of march of the glacier itself, any more than a black
beetle could run merrily along under the foot of the latest
President of the British Association.

If we turn to the alternative hypothesis of Mr. Belt, we are
in no better position.   His theory certainly has the merit of
boldness.

It is fair to quote it in his own words.   He urges that " at
the height of the glacial period, the bed of the Atlantic was
occupied by ice flowing from the north-west, from the direc-
tion of Greenland, reaching from the coast of Europe, either
at Brest or some other point further south, and damming
back the whole of the drainage of Northern Europe into an
immense lake of fresh water, which, at its greatest extension,
reached to a height of at least 1,200 feet above the present
level of the sea.   This lake was gradually lowered for some
hundreds of feet, probably by the deepening of a channel of
outlet, and then suddenly and completely drained by the
breaking away of the ice dam, causing an enormous flood or

débâcle " (*Journ. Geol. Soc.*, xxxii., p. 84). In order to complete the boundaries of his lake, Mr. Belt elsewhere postulates that Bering's Straits were occupied by another ice belt, while the communication between the Mediterranean and the Black Sea was not then cut, and he urges that by the gorging of the rivers the waters rose all over Northern Asia and Europe, thus drowning the big mammals, and when the ice barrier broke, the European lake discharged itself in a torrential fashion, and thus distributed the loose surface deposits of Europe as we now find them. This is assuredly a very bold hypothesis. If we try to realise the conditions of climate that would be necessary before the Bay of Biscay could be gorged with ice, or before ice on any great scale could exist in the sea in the latitude of the Pyrenees, while a temperate climate existed on the land, we shall measure some of the difficulties of such a theory. How could hippopotami and fig trees exist in Northern France with such a mass of ice close by that the sea must have had a temperature like that of the palæocrystic sea ?

The short European rivers would, with such conditions, be permanently frozen, as would the waters of the European lake formed by their being ponded back. Rain falling on the frozen ground would itself immediately freeze. To introduce the present conditions of Yakutsk into Western Europe when the mammoth lived here, would be a trial to our faith ; but to introduce those of Smith's Sound, and yet to make them coincident with a teeming animal and vegetable life, and a mass of liquid unfrozen fresh water, is indeed beyond all our powers of imagination.

We can no more credit Mr. Belt's explanation of the cause which distributed the valley terraces than we can one of Swift's imaginative tales. Such a theory is, as we think, at issue with the evidence, especially the evidence of climate furnished by the animal and vegetable remains. It also involves the creation of a lake so deep and vast in extent (for it must have reached to the level of the higher European plateaux where the fine loams are spread out) that we have no parallels with which to compare it, and it involves such a lake remaining unfrozen, although composed of fresh water, while the salt water of the Atlantic was gorged with ice

several hundred feet thick, and discharging itself along lines, such as the trough of the English Channel, where, so far as we know, there are no appreciable traces of the great masses of débris there ought to be. Such a flood would sweep the land of the greater part of its soft mantle, while the facts require an explanation that would spread such a mantle over the higher ground and thrust it into the heads of the lateral valleys, as we find it there.

But apart from this altogether, Mr. Belt's ice dam is a barrier apparently incompetent to prevent such a vast lake from draining unless it was of an entirely different nature to ice dams elsewhere. As Prof. J. D. Dana says : " There is no such damming, as far as is known, about Greenland, the sub-glacial streams being large and flowing freely to the sea ; and hence the practicability of damming the fresh waters in the way supposed may be doubted " (Silliman's *Journal*, 3rd ser., vol. xiii., p. 384).

However we try to explain, therefore, a water supply sufficiently developed to alter to any considerable extent the eroding character of the European rivers, we meet with failure. We cannot revolutionise the meteorological conditions of a whole continent without introducing portentous changes into the geography of the district for which we have no warrant. If we were justified in postulating such changes without proof they might help us with this problem, but they would create still greater difficulties in regard to other problems which are really concurrent.

In regard to the notion that the land was much higher and the flow of the rivers much more rapid when the terraces were formed than they are now, we are equally void of proof. Take the Thames, for instance. What evidence of any kind is there that the Thames, at the time in question, was a rapid and not a sluggish river, and that Gloucestershire and Oxfordshire were raised far above their present levels ? Even if this were so, unless we could largely increase the volume of the water in those rivers, we should still fail to move and carry away beds containing in places such very large pieces of flint, and such large masses of grey wethers.

Lastly, there is perhaps the most critical difficulty of all. I would ask, with the French geologists, if the rivers have

removed this vast mass of débris out of the valleys in the course of their handiwork, where has it all gone to?

Where are the deltas at the mouth of the Somme and the Seine or the Thames whose gigantic size would at once explain where all this material has disappeared to? Assuredly these are very proper questions to put, but they are not all. In the midst of the Rhine valley, near Freiburg, is the well-known volcanic pyramid called the Kaizerstuhl, which is covered with Loess. How came this there? how is it that it was not swept away (for it is largely composed of pumice and ashes) when the river was carving out the Rhine valley? and even if its volcanic nucleus was able to resist the water, how comes it that the soft mantle of Loess enveloping its flanks was not torn off? for it will be noted that such an obstacle as this mountain, standing in the midst of the river, would offer a tremendous buttress to its waters and be swept clean of all loose materials. What is true of the Kaizerstuhl is true of other old volcanoes in this district, of which Mr. Belt himself pertinently says : " The most ardent advocates of the theory that the Loess has been left by the river while it ran at higher levels will not, I think, suggest that the volcanic cones have been carved out by it, and yet excepting on that supposition their theory falls to the ground ".

Whichever way we approach the problem, therefore, we are constrained to one conclusion, namely, that the work of erosion done by the existing rivers, or by any rivers which the evidence justifies our postulating, has, except in limited places, been confined to cutting out their own narrow beds, and that they have not washed out of the valleys the great mass of materials which, according to the hypothesis of Lyell's scholars, once filled them up to the level of their higher terraces. We shall return to the subject again later.

We must now turn to another problem. Lyell, Prestwich and others not only claim that the rivers have scoured out the diluvium in the valleys to the extent and in the manner mentioned, but also that the whole of the diluvial deposits that now occupy these valleys more or less, were deposited by these rivers. Here again we have a conclusion which seems quite untenable.

There are substantially three different kinds of material being

deposited at this moment in river valleys—river mud, alluvium and peat. The explanation of peat forms no part of our subject at present. What I would take exception to is the continual confusion of alluvium with river deposit or warp. They seem to me to be essentially different in substance and in origin. The alluvium which forms the surface skin of nearly all valleys is almost entirely *humus*, or soil which has arisen from the decay of vegetable matter *in situ*, the work of earthworms, etc., *i.e.*, by the various agencies described by Darwin in his delightful work on the origin of soil, and has very little to do with river deposit.

Occasionally, when rivers break their banks, or when they overflow them in floods, they deposit a fine layer of warp, which becomes mixed with the ordinary humus, and to that extent sophisticates it, but otherwise the great holms and meadows which are so green and fresh are formed not of river mud, but of true humus. The two kinds of deposit can be at once discriminated if we compare the dark soil of a rich valley with the white or grey earth of most deltas, and notably of such silt as is found in the more recent deposits round the Wash and the estuary of the Humber, etc. What seems to me as clear as possible is that a large part of the alluvial carpet of our valleys is not a fluviatile deposit, and has nothing to do in any way with river action.

Let us now turn from the alluvial soil in the valley to the bed upon which it rests, *i.e.*, to the brick earth, sand and gravel which take up so much space in every European valley known to me, and which used to be known here by the excellent name *diluvium*, and are still so known to the French geologists. That the diluvium *as we now find it* is a fluviatile deposit is the conclusion of nearly every geologist known to me who is a believer in the methods and conclusions of Lyell.

To me no geological view seems to be more arbitrary and less based on induction. I shall have a good deal more to say of this deposit presently. Now I would merely remark that it seems to me inept to assign a fluviatile origin for a deposit which is not confined to the valleys, but which mantles the whole country very often for miles, quite irrespective of its contour, height or aspect. Secondly, that in no respect is it like the material now being made or carried or deposited by

rivers, *i.e.*, warp, delta-mud, "buttery clay," etc. As I have just described it, rivers, no doubt, have very largely cut their channels through diluvial beds, and in doing so have carried away a certain amount of gravel and clay and sand which once formed a part of the diluvial lining of the valley, and we thus meet with a certain amount of similar gravel and sand in the river beds to that we find on the banks on either side, but this has come from erosion of these same banks, and from the beds of diluvium which were once continuous and were cut into by the river in shaping a road for itself. To infer from the débris thus found that the diluvial mantle of the valleys was deposited by the rivers is, it seems to me, unmeaning language and to entirely ignore the kind of work rivers are doing and the kind of deposits they are making. As I have said before, nowhere in Europe, so far as I know, is anything like the diluvium being made and deposited by the present rivers.

The great beds of immense flint boulders among which the palæolithic flints occur which are not washed clean as in a river, but imbedded in ochreous sand and clay; the great masses of grey-wether sandstone, several feet long, having their edges generally unworn and (as Lyell himself says) when spherical owing their shape to an original concretionary structure and not to trituration in a river bed; the broken chalk of every size, from a fine powder up to fragments as large as a man's head—what rivers known to experience are depositing materials of this kind? " Many of these fragments," says Dr. Andrews, " though soft enough to write with upon a blackboard, have preserved with absolute perfection the sharp angles and edges which they had at the time they were broken from the cretaceous strata. It does not seem possible," he adds, " that they could have been rolled a hundred feet in the bed of a stream without losing their sharpness " (*op. cit.*, p. 183).

If these soft materials were deposited by rivers, they must have been rivers acting diametrically in the opposite direction to any rivers known to us now. They must have deposited vast depths of gravel, formed of pebbles of considerable size and of larger blocks, where they now only carry very fine sand; therefore they must have been much larger and

more rapid rivers than the present ones, *and therefore more effective as scourers.* On the other hand, as they deposited materials in thick beds, not merely in their deltas but all over the valleys and right up the subsidiary small valleys whence their headstreams came, they must have been *less effective as scourers* than the present rivers.

It has always seemed to me, therefore, very strange that those people who have championed the capacity of rivers to fill up their valleys from end to end with heavy materials have also attributed to the same rivers the faculty of scouring out these same materials. How the same river, which is a conservative agent along its whole course, can suddenly become a destructive and erosive one along its whole course is a mystery to me. Of course a river can and does undermine in some places, and throw down the materials which fall into it in another, and can be destructive when it runs swiftly and conservative when it runs slowly, the two operations occurring in different parts of its course; but this is not the suggested view, which is that the same river Thames which laid down the Thames valley gravels from Gloucestershire to the sea afterwards reversed the process, and proceeded to clear out the whole of the deposits so made. This reversal of the capacity of a river needs some revolution in its nature to explain it which is not forthcoming.

Again, when the supposed rivers were gorging these valleys right up to their fountain sources with the débris of which the diluvium is composed, whence did they derive it all? Not from the river bed itself, for directly a very thin layer was deposited the fundamental rock would be protected from erosion. Whence, I repeat, then, did they obtain the materials with which to fill up the valleys to such a vast depth with sand, gravel and brick earth as to reach up to the upper terraces? How is it, again, that in depositing these materials they did not deposit them according to their specific gravity, so that we should have a graduated series of them, beginning with the upper valleys and becoming finer and finer as we neared the mouth?

How is it possible for any one who believes in scientific methods to claim such a deposit as a fluvial one? Is it credible that it would ever have been so claimed if it had not

been for the far-reaching hypotheses which had been adopted as inspired dogmas, to support which the facts must be made to bend, and that having been so claimed by the greater prophets it was naturally claimed also by the lesser ones.

The puzzle is not only a very big one, but one also which grows with every fresh inquiry, and I am bound to say that as the facts appeal to me, the conclusions of Lyell and of Prestwich on the filling up of the valleys with diluvium by the pleistocene rivers involve a purely transcendental fancy.

The rivers, such as we know them, according to my view, began to flow when the valleys were mantled largely as they are mantled now with their diluvial covering. It was in this diluvium they formed themselves channels, in which for the most part they have flowed ever since. In the deposition of this diluvium they seem to me to have had no part or place. It was distributed, as we shall see presently, and as is held by so many continental geologists, not by fluviatile agencies, but by much more general ones, which, in many cases, covered not merely the valleys but the intervening plateaux and watersheds with the same materials.

So much for rivers as porters of soft and movable clay, sand and gravel. Let us now turn to the work of rivers as eroders of hard rocks.

In describing this process I prefer to begin by quoting one of the graphic sentences from the picturesque pen of Sir A. Geikie rather than my own words. He says: "In the mountain tributaries of a river we find the channels choked with large fragments of rock disengaged from the cliffs and crags on either side. Traced downwards, the blocks are seen to become gradually smaller and more rounded. They are ground against each other and upon the rocky sides and bottom of the channel, getting more and more reduced as they descend, and at the same time abrading the rocks over or against which they are driven. Hence a great deal of débris is produced, and is swept along by the onward and downward movement of the brooks and rivers. The finer portions, such as mud and fine sand, are carried in suspension, and impart the characteristic turbidity to rivers; the coarser sand and gravel are driven along the river bottom. . . . In transporting its freight of sediment a river performs a vast amount of abrasion.

In the first place, it rubs the loose stones against each other, breaks them into small pieces, rounds off their edges, reduces them to rounded pebbles, and finally to sand or mud. In the next place, by driving these loose materials over the rocks, it wears down the sides and bottom of its channel, which is thereby widened and deepened. The familiar effect of running water upon fragments of rock, in reducing them to smoothed rounded pebbles, is expressed by the common phrase ' water-worn '. Every stream which descends from high, rocky ground may be compared to a grinding mill ; large boulders and angular blocks of rocks disengaged by frosts, springs and general atmospheric waste, fall into the upper end, and only fine sand and silt are discharged into the sea " (" Geology," *Encyclopædia Britannica*, x., pp. 274, 275).

This is a true and a graphic description of what takes place in the rapid and tumultuous head streams of many rivers, and also where they pass through ravines and gorges, and it is a great temptation to illegitimately extend the lessons this phase of river action teaches us, until we make-believe that a mill which can grind wheat into flour can squeeze oil out of paving-stones.

In the first place, the size and shape of the stony débris are very material elements indeed in estimating their denuding capacity, and both the size and shape alter very much as we leave the turbulent cradles of the rivers and get down into their more placid waters. In regard to the very fine materials, Daubrée, the distinguished author of *Etudes Synthétiques de Géologie Expérimentale*, has shown that a limit is put to their attrition by the size and specific gravity of the grains. So long as they are carried in suspension they will not abrade each other but remain angular, and he found that the milky tint of the Rhine at Strassburg, in the months of July and August, was due not to mud, but to a fine angular sand (with grains about $\frac{1}{20}$ of a millimetre in diameter) which constitutes $\frac{2}{100000}$ of the total weight of water. Yet this sand had travelled in a rapidly-flowing, tumultuous river from the Swiss mountains, and had been tossed over waterfalls and rapids in its journey. He also ascertained that sand with a mean diameter of grain of $\frac{1}{10}$ mm. will float in feebly agitated water ; so that all sand of finer grain must remain angular. The same observer has noticed that sand composed of grains

with a mean diameter of $\frac{1}{2}$ mm., and carried along by water moving at the rate of one metre per second, gets rounded, and loses about $\frac{1}{10000}$ of its weight in every kilometre travelled.

Turning from sand to gravel, Daubrée found what indeed might be expected, but what is sometimes overlooked, namely, that the eroding quality of the stones rapidly diminishes as they lose their angularity. He found that after a journey of twenty-five kilometres angular fragments of granite had lost $\frac{4}{10}$ of their weight, while in the same distance fragments already well rounded had not lost more than $\frac{1}{100}$ to $\frac{1}{400}$ (Geikie, *op. cit.*, pp. 275, 276). Inasmuch as the attrition of the pebbles is by each other and by the rock they pass over, the complement in a measure of their denuding tendency, this shows how (even when the river remains an efficient carrier) the eroding tools rapidly become blunt and comparatively inoperative. Passing from the tools to the area over which they can work, we shall be constrained to confess that this erosion of hard rocks by rivers is a fluviatile function which has been outrageously exaggerated by many orthodox geologists. Water, when carrying along gravel and stones which are dragged or pushed over rocky beds, is no doubt an effective erosive and burnishing agent within certain limits, but those limits need a good deal of defining. Along nine-tenths of the course of every river the process in question is not going on at all, and cannot go on for two very good reasons—the first one being that the flow of the river is not sufficiently rapid and powerful to move the stones at all, and, therefore, to set the files and planes which do the work in motion; and, secondly, as I have said, almost every river known to me flows for a large part of its journey in a padded cushion of soft materials, which entirely protects the subjacent rocks from denudation of any kind.

Be it remembered that virtually all rivers, except mere transitory mountain torrents, flow through valleys which are mantled with soft materials, and they never, except in the upper reaches and in rapids, come in contact at all with the real skeleton of the valleys. And, so far as we can see, this must always have been the case since the present rivers began to flow. So that since the valleys were first occupied by their diluvial covering there cannot have been any material erosion

of their beds. This means that if these valleys were originally carved by rivers, it must have been by another set and another kind of rivers altogether to those we know now. But what possible evidence is there that they were so carved out? In the first place (and upon this there is an astonishing silence among the advocates of great river erosions), the outline of the rocky beds of the valleys wherever I have been privileged to see them is entirely different to any river-bed I have ever known or any river-bed I can conceive. They are torn and rugged and as remote as can be from the smooth outline which the present soft covering of the valleys gives them. How could any river, however armed and however powerful, cut out of the solid rock such valleys as were disclosed in the making of the great Lancashire ship canal, and as have been disclosed in so many other cases, as in railway cuttings, etc.? The thing is preposterous.

Again, what kind of a fall would be necessary to enable some of the longer and more important rivers we know to carry along with them graving tools sufficiently powerful to carve out these valleys out of their solid matrix? When I consider this difficulty again, I say the notion is preposterous. It was felt to be preposterous by some of the more active evangelists of the creed of transcendental denudation, so they invented an entirely new method of valley erosion by rivers. The erosion by the rivers, according to this view, did not begin with their headwaters and gradually work downwards. It began at the mouths of the rivers and worked upwards. It was postulated that it is the rule for the cataracts and falls on rivers to gradually recede in consequence of the continual weathering of their front face, and in this way any reach of a river which is limited by having a waterfall or cataract above it is continually working its way backwards and upwards. This view was, I believe, first enunciated by Playfair, and afterwards pressed by Dana and others, and is known as the theory of the cutting back of river valleys. It is very popular with orthodox geologists.

Playfair enunciates the theory in characteristically exaggerated terms. He says that " innumerable cataracts are entirely obliterated, and that those that remain are verging towards the same end ". I believe this theory or hypothesis to be

perfectly untenable, and that it represents the kind of science which many are obliged to shelter under who base their creed not on facts, but on dogmas. It is quite true that if we go down to Folkestone and walk along the shore we shall presently see a vast and tumbled undercliff which has fallen down, and is a fine example of denudation on a great scale in certain cases. It is quite true that in this instance the hard and stubborn strata, which formed a larger part of the cliff, were underlaid by beds of slippery gault clay which became gorged with water, and that this unsafe and unsteady foundation having given way the whole of the face of the cliff gave way too, and formed the ruin we see there.

The case just quoted from Folkestone is not an isolated one. Sir A. Geikie has brought together some other illustrative examples. Thus he says : " In the year 1839 a mass of chalk on the Dorsetshire coast slipped over a bed of clay into the sea, leaving a rent three-quarters of a mile long, 150 feet deep and 240 feet wide. The shifted mass, bearing with it houses, roads and fields, was cracked, broken and tilted in various directions, and was thus prepared for further attack and removal by the waves. . . . The undercliff of the Isle of Wight, the cliffs west of Brandon Head, County Kerry, the basalt escarpments of Antrim, and the edges of the great volcanic plateaux of Mull, Skye and Raasay furnish illustrations of prehistoric landslips. Of continental examples the well-known fall of the Rossberg, behind the Rigi in Switzerland, is one of the most remarkable. After a rainy summer in 1806 a large part of one side of the mountain, consisting of sloping beds of hard red sandstone and conglomerates resting upon soft sandy layers, gave way. Thousands of tons of solid rock suddenly swept across the valley of Goldau, burying four villages, with about 500 of, their inhabitants. In 1855 a mass of débris, 3,500 feet long, 1,000 feet wide and 600 feet high, slid into the valley of the Tiber, which, dammed back by the obstruction, overflowed the village of San Stefano to a depth of fifty feet, until drained off by a tunnel."

These are striking and interesting cases, but they are after all comparatively rare and unusual and dependent on very exceptional conditions. It is as ridiculous to apply the lessons

derived from these cliffs, where tough strata lie over rotten foundations, to the possibilities of ninety cliffs out of a hundred as it would be to measure the potential or actual physical power of every man by the example of Sandow. Let us now apply this lesson to our problem. The Falls of Niagara happen to be at a point where the physical qualities of the rocks are peculiar. These rocks consist of eighty feet of hard limestone superimposed upon eighty feet of friable shale. The shale is very easily disintegrated under the influence of the water and spray blown against it and of frost, and as it disintegrates and is undercut the limestone blocks above, which in places have overhung for as much as seventy feet, give way and fall into the hollow below. " The bed of solid limestone is not eaten back by the current flowing over it. It breaks away by its own weight because its natural support is removed, not by the running water above, but by the splash of the spray from below dissolving the underlying shale. It is the limestone which has protected the shale and prevented it from being washed away long ago." In this way the Falls of Niagara are apparently continually receding, but Niagara is, if not unique, a very rare instance indeed of this kind of work done by a river. I know of no other instance to compare with it. On the contrary, it seems to me as plain as can be that when the rocks are continuously hard or tough, as they are in most places where the waterfalls we know exist, there is no recession at all going on, or one that is quite inappreciable.

It is very hard indeed to see how there can be any, since the necessary graving tools in such instances are very uncommonly present, and where they are, they are carried along in the sweeping current without being able to act efficiently. The recession of Niagara is due to the spray and water beating against the soft shale, but this beating of spray and water against crystalline and other hard rocks is as ineffective in denuding them as a spring breeze is in eroding a man's bald head. I have travelled much and seen any number of waterfalls, and the surprising thing to me is the way in which they have preserved their old rough lines and rugosities in most cases, and how little of even polish there is among them. A few walks about Grindelwald, a very ordinary pleasure resort,

will supply any number of instances, while a journey through the lake districts will supply some quite as good.

Ruskin says: " The one vulgar and vast deception of Niagara has blinded the entire race of modern geologists to the primal truth of mountain form, namely that the rapids and cascades of their streams indicate, not points to which the falls have receded, but places where the remains of once colossal cataracts still exist, at the places eternally (in human experience) appointed for the formation of such cataracts, by the form and hardness of the local rocks. The rapids of the Amazon, the Nile, and the Rhine, obey precisely the same law as the little Wharfe at its Strid, or as the narrow *rivus aquæ* which, under a bank of strawberries in my own tiny garden, has given me perpetual trouble to clear its channel of the stones brought down in flood, while, just above, its place of picturesque cascade is determined for it by a harder bed of Coniston flags, and the little pool, below that cascade, never encumbered with stones at all " (*Deucalion*, p. 75).

There are cases, no doubt, where the rocks are much jointed and have been therefore broken up long ago into polygonal blocks, and where in great floods the blocks are shifted and moved on. These are not so frequent in waterfalls, etc., however, as is sometimes thought, but it is very different when we have to deal with the case of a homogeneous hard crystalline bed, even sometimes when that bed is only limestone, as, for instance, the famous waterfall at the Staubbach, of which Ruskin wrote that it had not so much as cut back through the overhanging brow of its own cliff. There are numberless spouts and waterfalls in Switzerland where the water rushes to the very verge of the deep perpendicular wall of rock, over which it falls without its having worn back the edge even a yard since the valleys were first formed, or rather since the stream which tumbles over began to flow.

The same is true again in regard to the shifting of cascades and forces and cataracts. Of these I have seen a great number and observed them closely. They no doubt furnish examples of local denudation on a considerable scale. Pot holes and giants' cauldrons occur in such places very frequently, where some stone or a mass of gravel has got entangled in a hollow and is swirled round and round by the rushing water, and in

this way drills a polished hole as effectively as a regular boring tool. In other places the stones are worn smooth and have their backs rounded and their sides hollowed by similar means; but this, again, is much exaggerated. So long as the surface of the rocky bed is rough it offers resistance to the rounded stones that come along and a good deal of erosion takes place, but directly they have been shaped into curved surfaces and smoothed and polished they offer no such resistance, except where there has been a block or an entanglement.

Let me quote in support of this view some experienced observers.

" The phenomena of cataracts," says Mr. W. D. Conybeare, one of the greatest of the old masters, " are inconsistent with the fluvial hypothesis. The fluvial hypothesis requires me to believe that since the emergence of our continents the atmospheric drainage has commonly furrowed them into valleys hundreds of miles in length and hundreds of feet in depth— that the streamlets forming the Thames, for instance, have done this. Cataracts, indeed, appear generally to have undergone surprisingly little change from the earliest periods to which history extends. The cataracts, or rather rapids, of the Nile above Syene, when examined by the sçavans of Buonaparte's expedition, agreed pretty closely in locality, feature and extent with the description given by the Grecian father of history. I have always inclined to consider the cascades of Tivoli as another evidence of the slight changes effected in this way during a long series of centuries. . . . All the localities of this scene still appear the same as when its beauties inspired the muses of Horace and Statius some eighteen centuries ago; the voice of the sibyl, the ' domus Albaneæ resonantis,' still re-echoes with the dash of the falls beneath; though did rivers travel at the rate the fluvialists think they do, the said falls must surely have removed far beyond earshot of the old sibyl long ago. . . . The natural phenomena of the spot appear to prove that the place of the great cascade has been stationary, or nearly so, from the moment when the river commenced its course through the valley of excavation previously traced out for it by some cause far different from any action of the river itself. The circumstances of the spot are the following. The Anio above Tivoli flows gently on-

wards towards the edges of the precipice, through a gorge of Apennine limestone of the oolitic period. Near the entrance of Tivoli a dyke has been constructed across it, diverting a part of its waters through an artificial tunnel on the left or southern side, and thus conducting them so as to issue in several artificial *cascatelli*. One of them, by its relation to the domain of the great Mæcenas, shows that no change has taken place since his time, along the line of its descent, from his palace to the bottom of the valley beneath, excepting the deposition of travertine; the waters of the Anio, while foaming in a state of precipitation, have always deposited travertine; and this travertine, accumulating on the bar of limestone over which it fell, as may especially be seen at the great and only real cascade of the Grotta di Nettuno, immediately became a defence against all further erosive action of the river on the subjacent Apennine limestone. This perpetually increasing shield of travertine would probably go on accumulating more rapidly in its upper parts than the agitated state of the bottom would allow below; and hence periodical breakings away of its unsupported over-growings would take place, as of the curling edges of drifted snow. But this failure of support would not affect the inferior sheets of travertine in immediate contact with the limestone; these, once formed, will have remained from the day of their formation, arresting all further destruction of the stratified rock beneath. From the base of the cascade to the plain of Rome is about a mile; and in this mile the river descends a valley, narrow at its base and flanked on both sides by slopes of moderate inclination, most steep at the *cascatelli* on the left bank, and on the right bank nearly opposite to them. Now, it is observed, had this cascade been working gradually backwards through the eternity of the fluvialist theory, it must have deposited travertine all along the gorge it was forming at each successive station which it occupied from time to time, just as at its actual station the Grotta di Nettuno; every part of the gorge below this point should have been a precipitous ravine uniformly incrusted with travertine such as now covers the site of the actual cascade. Instead of this, we have a valley included by gentle slopes except at its upper extremity, where its sides for a short interval become more steep; nor is there a single particle of the travertine which, on

the theory, ought to have prevailed through its whole extent, except in the neighbourhood of the *artificial cascatelli*, which, of course, must necessarily produce it, just as the natural cascade does " (*Phil. Mag.*, ix., pp. 266-270).

Turning to a later and not less keen and incisive reasoner, the late Mr. John Murray says : " Those who dwell near the rushing water of ˌcataracts are unconscious of the abrasion of a single foot or inch within the term of man's memory. The Linn of Dee, in Braemar, is a small fall caused by the whole river forcing itself through a natural cleft in its bed not three feet wide. Macgillivray says of it : ' Great as the force of the stream must be, it has failed to wear off projecting angles or to straighten the passage. Considering the power of running water, and especially the wonderful effects it is represented as producing, we naturally think it strange that this fissure in no very hard rock should remain so little changed. The Dee, with all its floods, and many they have been, has rushed along this narrow rent, I suppose, some thousand years without so much as fairly smoothing its sides ' (Macgillivray, *Natural History of Deeside*). The frequent growth of water plants, mosses, sea-weeds, etc., on the very surface washed by rapid currents ought to create doubts as to the prevalent notion " (Murray, *op. cit.*, pp. 58, 59).

· Mr. D. Mackintosh mentions similar cases. " Thus," he says, " about a mile and a half to the south of Newtown there is a short deep gorge terminated by a high cliff with a picturesque waterfall. But the latter cannot be credited with having excavated the gorge backward, for the following reasons : the breadth of the stream and of its channel above is not equal to the extent of the cliff, and the continuity of the face of the cliff has not been broken or indented by the waterfall, but presents the appearance of the inner precipice of a previously scooped out hollow. The same remark applies to many of the waterfalls of Wales, which are merely falls or slides over transversely-continuous cliffs. The waterfall near Aber furnishes a striking instance of a stream tumbling over a long and continuous cliff of hard rock, which forms the inner boundary of a valley containing marine drift. At the bottom of this valley, and distinct from its general outline, the stream

flows along a clearly-defined channel. A similar channel may be seen above the fall; both are the work of the stream, but the falling over the cliff is a mere accident in its history," etc. (*Geol. Mag.*, iii., p. 395).

Again he says : " Having had very little faith in waterfalls as denuding agents, I was not much surprised to find that those of Barrow and Lowdore in Cumberland still seemed to be running over the old sea escarpment much in the same fashion as they probably did when the plain of Keswick was an inland sea. These waterfalls, according to the subaerial theory, ought ere now to have receded a considerable distance from the eastern escarpmental boundary of the plain of Keswick. Watendlath beck gives no evidence of its being more than a usurper of Watendlath ravine."

In a paper by Mr. Goodchild on " Ice Work in Edenside " there is a very good analysis of the position I am maintaining. He says : " Some good examples of the different rate of weathering of the same bed of shale where exposed to the direct action of running water and when affected only by weathering are found about the waterfalls or 'fosses' in the Dale district. Under the waterfall the shales that there underlie the sandstone, as this does the limestone of the foss, are kept in the condition most favourable for their rapid disintegration, so they are quickly cut back beneath the harder beds that form the edge of the fall. But at the outer end of the ravine, caused by the gradual recession of the waterfall, subaerial denudation has accomplished so little, notwithstanding that a rapidly flowing stream is at hand, that the difference between the rate of recession of the foss and that of the sides of its ravine in one case is about as forty to three. In other words, while that waterfall has cut back forty feet, each cliff it has left has receded only eighteen inches.

" The particular instance here referred to," says Mr. Goodchild, " is doubtless an extreme case where the beds overlying the shale are more than usually durable; but it serves to prove that even where there is a rapid stream flowing the denudation of shale does not go on very rapidly unless the stream actually flows close to the outcrop. Where limestone is the rock directly overlying the shale, this last is usually

cut back much faster because the surface water finds an easy passage into it through the weathered joints of the harder bed above. . . . If then so little denudation of a rock as easily worn as shale has been accomplished in post-glacial times by the rapid streams of the Dale district, where these streams are absent we should be prepared to find that the rate of denudation is so slow as to produce effects that are hardly perceptible. . . . The thinner kinds of sandstone, especially where these are much split up by beds of shale, seem to go to pieces very readily, but upon the more compact, blocky and little-jointed kinds ordinary weathering seems able to produce very little effect."

My own experience is precisely the same in regard to waterfalls, whether I have examined them in Sweden or Switzerland or Cumberland. They seem to me to present the most potent evidence of how little eroding takes place when they occur amidst hard rocks. The very drapery of ferns, mosses and trees, which covers every bit of the surface where the water does not actually beat, shows how little the front of such clefts is worn away, while in islands like that of Elephanta on the Nile, and similar islands on the Indian rivers containing very primitive monuments, we have a witness of how very slight is the effect of the river erosion. The cataracts of the Nile and the Falls of Trolhätten present us with a similar lesson. The fact is this theory of the recession of waterfalls and cascades is a vast induction, not based upon the great majority of cases, but upon a few exceptions which can be very easily explained as Niagara is explained.

The existence of cataracts, rapids and waterfalls is in itself a very commonplace proof that the valleys where they occur are not the result of slowly eroding waters. The latter would carve out channels with continuous slopes, and affording a roadway of least resistance for their waters, and would not cut out a succession of great steps with intervals marked by broken and dislocated strata and rushing waters, and the intervening reaches by smooth and untroubled water. The very fact that so many valleys have this step-like contour —the successive steps being often occupied and marked by lakes—is a remarkable piece of elementary evidence which the extreme school of uniformity has too much neglected.

It would seem plain, therefore, that the notion that rivers have cut back their valleys is as futile as that they have eroded them in their march downwards towards the sea. In neither case has the physical possibility of their doing so been shown, while in both the theory seems surrounded by insurmountable difficulties.

Note.—In regard to the actual contour of the solid surface of valleys below the soft covering which disguises it, discussed on page 274, I would refer to a description by my friend Mr. Mellard Reade of the contour of the valley of the Mersey under the boulder clay. He says if the deposits of sand and clay were stripped from the triassic rocks, we should behold a landscape of a much more varied character than we now see (*Geol. Mag.*, 1896, 490).

CHAPTER VII.

RIVERS AND THE SEA AS DENUDING AGENTS.

" Men of science, to render themselves worthy of the license given them in what they communicate to the world, should carefully distinguish between truths which are definitely established by unquestionable proof and ideas which are as yet mere problems or opinions."—Virchof, *Freedom of Science.*

WE have examined the doings of rivers when they are engaged as porters in performing their first duty and purpose, namely, securing for themselves beds upon which to flow which shall be as uniform in slope and in velocity and as free from friction as possible, and also their intermittent acts under certain local conditions upon their banks. We have also collected a considerable amount of evidence to show how little erosive effect rivers exercise upon the rapids or falls where their course is interrupted, and how favourable the conditions must be when they are effective agents in cutting back their beds. We will now turn to one of the duties which have been assigned to them, one which is so assigned in perhaps the majority of geological manuals, and which I consider to be absolutely beyond their power, namely, the capacity to force themselves through solid and continuous masses of consolidated strata, sometimes consisting of the hardest crystalline rocks. That is what has been meant when it has been laid down that rivers are competent to excavate their valleys, and that they have, in fact, excavated them.

The question involved is a very old one. It was debated in the very early days of geology, before Hutton and Playfair, who are sometimes quoted as its originators, were heard of. Ovid, in an often-quoted passage, may be said to have been the originator of the idea, which in the last century was again taken up by Woodward and Bourguet.

In his *Lettres Philosophiques*, p. 181, Bourguet argues that the regularity of the angles formed by their bounding hills is a proof that the valleys have been cut out by currents of water, the salient angles on one side of a valley corresponding exactly to the re-entering angles on the other, and the angles in the larger valleys being more acute than in the smaller ones. To this De la Métherie replied that the fact is not true in the case of valleys formed in primitive rocks, in which the valleys are most irregular. There is generally a principal valley, no doubt, but its tributary valleys join it at all angles, and it cannot be said that they have salient and re-entering angles, for very frequently where such an angle ought to occur there we find another valley. This may be easily seen in the Alps and the Jura. De Saussure has remarked this in several places, and Pallas says emphatically : " Ce n'est point dans ces pays élevés qu'il faut chercher des preuves de l'assertion du philosophe *Bourguet*, renouvelée par *Buffon* sur les angles correspondants des montagnes, qui d'ailleurs souffre bien des exceptions dans les chaînes granitiques, et même souvent dans les montagnes des ordres secondaires " (*Observations sur les Montagnes*, p. 42).

The regularity referred to by Bourguet, as De la Métherie shows, is limited largely to those valleys in which we have deep deposits of loose materials which have been shifted by the streams running through them and rearranged .in this uniform fashion, and even if the law were universal it would not sustain Bourguet's view, since there is no reason why valleys should not have this regular distribution of their angles at their elbows and recesses, even when the result of subterranean movements (De la Métherie, *Théorie de la Terre*, v., p. 416).

Let us pass on, however. It was Hutton who, in his *Theory of the Earth*, published in 1795, carried this view to its most extravagant length, and this explains why he has been so generally deemed the father of modern uniformitarianism. He would perhaps have conceded a certain original unevenness of surface to the earth, but he seems to argue in chapter after chapter that all the valleys and all the ravines, not only of mountain countries, but also of the plains, are directly due to the continued and sustained action of streams and rivers, to

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which he attributes not only the excavation of their beds but also the transport of the materials which were carried away. He contests even De Saussure's very moderate departure from his views where, when speaking of the Val d'Aosta, the latter says : " Je crois pouvoir conclure de là, que cette vallée est une de celles dont la formation tient à celle des montagnes mêmes, et non point à l'érosion des courans de la mer ou des rivières. Les vallées de ce genre paroissent avoir été formées par un affaissement partiel des couches des montagnes, qui ont consenti dans la direction qu'ont actuellement ces vallées " (*vide* De Saussure, *op. cit.*, par. 960 ; Hutton, *op. cit.*, ii., p. 397).

Hutton's conclusions were emphasised by Playfair, whose words are : " Rivers have cut and formed not their beds only but the whole of the valleys, or rather systems of valleys, through which they flow, and which is demonstrated on a principle which has close affinity to that on which chances are usually calculated ".

This extravagant position was adopted almost in its entirety by the wild men who secured the control of the Geological Survey on the retirement of Murchison—Ramsay, Beete Jukes and others—and is the chief view which has been for many years pressed upon the ingenuous youth of this country as a settled principle in geology. From the English Surveyors the view passed to those of India, and in the *Manual of Indian Geology*, by Blanford and Medlicott, it is stated in its most naked and extreme form, and it is apparently seriously argued that in the Himalayas every furrow, ravine and valley is the result of denuding forces, of which rain and rivers are the most potent.

This was not the view of the older masters, whose pupil I claim to be, and before whom some of the wild modern men ought to take their hats off.

Speaking against the excavation of valleys by rivers, Conybeare says that " the configuration of valleys, and especially the intersection by transverse valleys of continuous longitudinal valleys, themselves opening possible outlets to the drainage, is inconsistent with the theory which assigns the drainage of the atmospheric waters as the excavating agent ".

Sedgwick argued in 1843 that the erosion of rivers and

torrents, however indefinitely continued, could not account for the hollows and inequalities of any one of our mountain chains; that in instances almost without number we find streams making their way through clefts and gorges of solid rock, and escaping towards the sea on one side of a chain, while nature offers them an easy and uninterrupted line of descent on the *other* side, and that the configuration of no high country yet examined is in accordance with this theory (*Geology of the Lake District*, p. 7).

I need not here quote from Murchison, who preached the same sermon all his life, and whose judgment on these matters was so sane. I will rather quote Lyell, who, in the last edition of his *Elements of Geology*, says: "Many of the early geologists, and Dr. Hutton among them, taught that rivers have in general hollowed out their valleys. This is, no doubt, true of rivulets and torrents, which are the feeders of the larger streams, and which, descending over rapid slopes, are most subject to temporary increase and diminution in the volume of their waters. . . . But the principal valleys in almost every great hydrographical basin in the world are of a shape and magnitude which imply that they have been due to other causes besides the mere excavating power of rivers. . . . We may suppose the position and course of each valley to have been originally determined by differences in the hardness of the rocks, and by rents and joints which usually occur even in horizontal strata " (*op. cit.*, pp. 70, 71).

Let us turn from authorities to facts. Rivers, as is familiar to everybody, have two phases—one a boisterous one, where their beds slope rapidly and where their current is fast, which is mainly in their headstreams ; and, secondly, a more gentle and equable and regular flow, where their beds are not so much inclined.

Now, it is a remarkable fact that some of the most difficult work which has been done in excavating and cutting out gorges and ravines through which rivers now pass (whatever it was that did it) has been done not in their upper reaches, where they are not only rapid but armed with stones, etc., but half way down their journey, where they are running smoothly and gently, and where the water is merely travelling by itself and unloaded with heavy débris. It is in this way that gorges

like those of the Rhine, of the Nile, of the Danube, etc., occur at points where the rivers have neither a boisterous flow nor are loaded with stones. I have already tried to show that water when thus unloaded is as incapable of eroding hard rocks which are not soluble as my tongue is of wearing away my teeth, or the water flowing through an iron pipe is of wearing its way through it. I will, however, support my previous arguments by the opinions of better men than myself. Darwin says : " There is good evidence that pure water can effect little or nothing in wearing away rocks " (*Origin of Species*, p. 283).

Tyndall, in this behalf, says : " The denuding power of water is very slight ". Speaking of the valley of Hasli, he says : " A million winters may have acted upon these scarred and fluted rocks, and still the scars and flutings are as distinct as if they had been executed last year. We trace them down to the valley of the Aar, a river which has been rushing for these ages through the valley, and the smoothness of its operation must impress us with the comparative feebleness of denudation by water. The same is the case with the polishing, etc., in the Rhône valley " (*Phil. Mag.*, xxiv., p. 171). Speaking of other valleys north of the Alps, he says : " What air and water have accomplished since the disappearance of the glaciers are mere scratches of the tooth of time in comparison with the mighty furrows which had been previously ploughed out " (*ibid.*, p. 172).

We cannot lean over the parapet of a bridge anywhere and look down upon even a rapidly flowing river (except in times of flood and spate) without being struck by the way in which the stones and river bottom are covered with slime and green algæ, showing how slight, if any, is the effect of unloaded water on consolidated rocks or hard stone. We shall be equally struck if we examine the abutments of very old bridges, where the mason's marks often remain, or the surfaces of very old groins. Take, for instance, those of the Roman bridges on the Tyne, or of the Roman quays and the faces of the stones of the Great Cloaca on the Tiber.

How very slight this abrading power of running water is, even in the headwaters of streams, may be seen again in a great many faces of rocks in Switzerland and elsewhere, where

more or less continuous streamlets and brooks have been running for untold ages without apparently having softened in the least the asperities of the beds over which they flow. Of this remarkable examples may be seen from such a homely vantage as the hotel verandahs at Grindelwald. It is the same with the beds of a great many rapid streams where little or no gravel is found and where the slaty bottoms retain their corrugations and angles, although the streams must have been travelling along them from the first arrangement of the surface contour of the country.

Deluc, Dolomieu, Raimond, Brongniart, etc., have all urged that rapid rivers do not erode their walls and channels, but cover them with a rich vegetation of mosses, confervæ, etc. This is strongly urged also by an anonymous writer in the *Edinburgh Review.* He says : " We see rocks exposed to the fury of the waves, or to the force of rapid currents, covered with the humble forms of vegetable and animal life. It is idle to pretend that the force of water which cannot wash a limpet or an alga from a rock can cut through the solid material of which that rock is composed " (*Edin. Rev.*, vol. cxlvii., p. 366).

And yet we are to believe that the Rhine, for instance, when half-way down to the sea has cut out the Lurley gorge and forced itself a passage through the Seven Mountains.

But suppose we grant that rivers composed of unloaded water could carve and cut passages for themselves through long and continuous masses of solid strata, how are we to explain their doing so in the particular way in which so many gorges are cut ? No doubt in a deposit like the Chinese Loess, with its peculiar internal structure, there is a tendency for it to weather in a way that leaves flat perpendicular faces to the Loess cliffs through which many of the roads in North China pass, but there is no analogy in this to rivers scooping out their channels.

Nothing is more easy than to find empty beds of streams which have been made by water or which are being abraded at this moment, some in soft and others in hard materials, and nowhere, so far as I know, can we point to an instance where water, loaded with gravel or otherwise, has cut out for itself a channel with straight-up sides or with anything like

the contour of these gorges, and it seems to me that those who appeal to river action as the cause of such features have entirely abandoned inductive methods. They ought to prove the mechanical capacity of their tools before appealing to them.

Whether we examine the meandering course of brooks which have left their beds in the meadows or the scars found on the sides of some mountains by torrents, we shall notice, as we should in fact expect, that water in motion shapes its course with curved lines where "least resistance" is the thing aimed at and secured. The banks and the bottom of these stream beds have continuous slopes and curves from the surface right round the bed. It has never been shown how, by any laws of dynamical force, water, loaded or unloaded, could begin and cut out a ravine, like a Norwegian fiord or a Colorado cañon, with clear, angular edges and straight, perpendicular sides, generally marked, too, by ledges and platforms at intervals, as if some gigantic knife or saw had been used in the process. It is no use saying that rivers run through these gorges and that therefore they must have cut them, any more than it would be to say that because a canal runs from Manchester to Liverpool its bed was hollowed by itself, or that because men are walking along the streets therefore they have made them by the process of walking over them. The argument is as inept in the one case as in the other. I shall have much to say presently on the very different methods by which these cañons and rifts have probably been made. At present I am chiefly exercised to show that they have not been cut out by rivers. The magnitude of the operation is hardly realised. Many of the great river valleys display miles of lateral precipices, rising often to heights of 1,000 and 2,000 feet above the water, almost invariably as smooth and even as the walls of a house. How can we appeal to water to do work which is really like the process of sawing a piece of wood or marble with a saw, or cutting a loaf with a knife, and involves the formation of steep, perpendicular sides of hard rock, clean cut down and straight, in sometimes very narrow rifts, extending for hundreds of miles? The easy way in which the capacity of water for undertaking and completing dynamical work of this kind is taken for granted without any

proof is simply staggering. To those who have studied mechanics it is not merely improbable, but impossible. It is quite true that in many cases streams and rivers flow at the feet of perpendicular scarped cliffs. This is true enough, but in many other places these inland cliffs and scarps extend for scores of miles without any river or stream running at their feet at all, and the fact of rivers sometimes running along the bottoms of cliffs is no more proof that they cut those cliffs down than that the porridge I ate this morning excavated the basin in which it was served. "No one," says Mr. J. Murray, "has observed this sawing process in operation, not even in places where water exercises its greatest force, as at the cataract at Schaffhausen or the Falls of the Clyde (and he might have added the Falls of Trolhätten). . . . There is no instance to be found in any part of the world of water, even with the help of gravel and stones, cutting down a clean, smooth, vertical surface in hard rock."

The cases of water scooping out hard rocks which have been quoted by Scrope and Lyell, and by those who have repeated their words literally, as illustrations of the power of water to erode valleys, and to do so in more or less hard materials, seem all to be irrelevant. The only ones of any importance, apparently, which Lyell could find were three in number, and all three are merely cases of soft and disintegrated beds swept away by tumultuous waters. Thus it was with the houses swept away by the flooded Arno referred to by him in the *Principles*, i., p. 354. These houses were planted on beds of porous tufa. Similarly, in the case of the famous ravine at Milledgeville in Georgia, which has done yeoman's service for the erosionists, and which was excavated fifty-five feet in twenty years. The ravine is described as cut into beds of sand and clay, red, white and green, and, as Mr. Murray says, the stream has only done on a large scale there what any shower of rain does on a ploughed field by dissolving the clods into mud.

These are cases of mere porterage and not of excavation. The only cases presenting any serious difficulty are those quoted by Scrope and Lyell from the volcanic district of Auvergne and Etna. In regard to the case cited from the former district, namely, the passage cut in a columnar lava

current by the river Sioule in Central France between granite and columnar trap, it is accounted for, says Murray, by the current penetrating between the joints and separations of the lava columns, by which in process of time it has sapped them and swept them away like a row of skittles, *but it has stopped short at the original granite bed of the Sioule.* In regard to the instance quoted by Lyell from Simeto, at the foot of Mount Etna, the same writer, speaking *on the authority of "a trust-worthy scientific observer on the spot,"* says that while the lava on which the river now flows is comparatively hard, as Lyell says, the upper beds which the river has removed are scoria-ceous, "being a mere froth of the fiery mud," and offering slight resistance to running water. Even the more compact bed has little tenacity, and from the nature of its aggregation is very brittle and more easily battered to pieces by fragments washed down by the stream than a similarly compact rock of sandstone or limestone. "*The Simeto makes little or no progress now in the lowering of its bed*" (Murray, *op. cit.*, pp. 60-62). The fact is that no illustrations can be more misleading in this behalf than those derived from highly volcanic districts, where scoriaceous lavas and disintegrating ashes are so often disguised features of the beds.

No one doubts the disintegrating and carrying powers of water under certain conditions. There cannot be doubt, again, that when the flow of a river is fast enough, and its contents are able to move gravel or boulders or masses of stone suffi-ciently fast with its current, that these stones act as potent hammers and chisels and tools, and not only wear down the rocky bed on which they are rolling but also round and abrade themselves in the process. They thus polish, striate, groove and excavate rocks, and they convert the mass of angular débris of which they are themselves formed originally into more or less rounded boulders. Of this there can be no doubt. It is an elementary fact, but because it is an elemen-tary fact its real potency has been greatly exaggerated.

In the first place, we must remember, as I have previously urged, that rivers do not flow for the most part on rocky bottoms at all. Much the greater part of them for much the greater part of their course, as we have seen, flow in beds of soft materials which separate and cut them off from the

rocky foundations of the valleys, and very effectually protect them from wear. So far as we can judge, they have always so flowed since they were rivers, and when we can lay bare the rocky bottoms of the valley underneath this soft covering they testify by their rough and irregular surface that they were not shaped by the rivers. This fact has been largely overlooked by those who have discussed the river erosion of valleys. They have argued as if the stony bottoms of the valleys had the same soft contours as their cuticle of soft materials, and were always readily accessible, and therefore available for denudation.

The only parts of a river where erosion can in fact be said to be going on at all so as to affect the stony skeleton of the valley are in its headstreams, where the water rushes rapidly and torrentially, and is also loaded with gravel and other fragments of rocks, or when it rushes through gorges or cataracts in a tumultuous way and is similarly armed.

The gravel and the boulders when carried along by the water no doubt have an eroding tendency on the hard beds over which they flow. They can and do smooth them down and furrow them, and act as files and planes upon them. This is an elementary every-day fact, but it is very local, and applies almost entirely to the headwaters of rivers. Even this, however, must not be exaggerated. Anyone who has stood on the bridge at Lucerne and watched the Aar flowing at a mighty pace over its loose stony bottom without displacing a stone, or has noticed how many of the stones and stony beds of very wild cataracts and rapids are covered with green algæ and readily detached weeds, will realise how rapid and full the stream must be to produce really notable effects of this kind.

" Mr. Kelly," says Mackintosh, " the eminent antiquarian of Yealmpton, who is almost as familiar with some of the rounded boulders in the bed of the stream which runs under Ivy Bridge Viaduct as with the faces of some of his own family, has assured me that they have not moved an inch during the last twenty or thirty years " (Mackintosh, *op. cit.*, pp. 273, 274).

" If the atmospheric theory of the origin of the passes be correct," says Mackintosh again, " why is the stream at the

bottom of Llanberis Pass not carrying away the fallen blocks instead of allowing its bed to be raised by them, and gurgling amongst them in continual danger of losing its identity? Was the stream larger at a former period? It is questionable whether the watershed of the pass could ever have supported a stream much larger than at present" (Mackintosh, *op. cit.*, pp. 24, 25).

These examples show how careful we must be not to exaggerate the potency of streams even when armed with stones. Not only so, but it is perfectly plain that this kind of erosion must cease directly the river ceases to have enough propulsion in it to move the stones. If this erosion were a perpetual operation in the upper parts of streams instead of an accidental one, it would quickly excavate deep troughs for them, which would be perpetual fissures full of water, with their beds below the general bed of the river. It would seem, in addition, that the great mass of fine material which eroding rivers carry comes from the disintegration of the stones in the mill and not from the mill itself, which, having had its surface smoothed, offers small opportunities for erosion.

I will now quote the conclusion of the very famous and experienced Swiss geologist, Studer, who, after fully granting the erosive effects of rivers when in their torrential moods, goes on to say: " From time immemorial the falls of the Rhine, the Toccia and the Aar, and the cataracts of the Rhine at Laufenburg and those of the Danube and the Nile, have not changed either in place or form. When thus we see a river traverse solid rocks, such as compact limestones, granites or porphyries, while at the same level it might have cut itself a way through softer rocks, we must be convinced that its course has not been impressed upon it by erosion. The Rhine at Sargans had to surmount an elevation of only twenty feet above its highest level in order to throw itself in a straight line into the lake of Wallenstadt; why, then, should it have taken its course, making a bend and traversing the calcareous mountains of the Schollberg and Fläscher Berg, if these mountains had not offered it a more ready passage of a different and more ancient origin? Why should the Simme near Wimmis have forced a passage through the limestones of the Burgfluh, when between the latter and the Niesen there

were only schists to be traversed? Why should the Sarine have hollowed out its channel by the long limestone defile from Rossinière to Montboven, when to the left there was the depression of the Nosses, of which the flysch rock presented much less resistance? The impossibility of explaining by erosion these ravines, which are evidently large crevices, as also the relation existing between the longitudinal valleys and the strike and dip of the strata, are facts long since established by science" (*Phil. Mag.*, fourth series, xxvii., pp. 486, 487).

As long ago as October, 1829, De la Bêche, a geologist of great experience and judgment, published in the *Philosophical Magazine* an article on the excavation of valleys, in which he says, *inter alia:* "It seems utterly at variance with the relations of cause and effect to suppose that valleys, properly so called, could have been formed either by the discharge of lacustrine waters or by the rivers that now run or could ever have run in them" (*Phil. Mag.*, vi., p. 242). Turning to the valleys of excavation, as he calls them, of Devon and Dorset, where the former continuity of the strata on both sides is most obvious, he says: "At the bottom of each of these valleys we find a small stream, the natural drain of the land. Could these streams have cut out such valleys as they now flow through? If there be any true relation between cause and effect, they could not." Speaking of the valleys of Lyme-Regis and Charmouth, he says: "In the bottom of each valley is a little stream. . . . To such insignificant streamlets and the rain waters, which acted in conjunction with them, the advocates for the excavation of valleys by actual causes would refer the whole."

Speaking of the river Arun in Sussex, Martin says: "It is strikingly obvious that the stream has been employed in filling up, and not in excavating, the valley in which it runs"; and he goes on to say: "It is extraordinary that some naturalists still advocate the doctrine of the excavation of valleys and river courses by the abrasion of their own waters and the constant operation of existing causes. But these causes not only will not apply to the peaceful streams of more level districts, but are also entirely inadequate to the effects ascribed to them, though operating (as we know they have not done without interruption) through all time" (*Geology of West Sussex*, p. 71).

"I maintain," says Murchison, "that the clearest proofs abound that in numberless cases rivers have simply availed themselves of the courses prepared for them by previous breaks in the rocks, opening depressions along which the waters have passed. Take one of the largest of our European streams, the Danube, and trace it from its source in the flat plateaux of Central Germany, in which it rises, and you see that, whilst it never can have been a torrential stream, it simply maintains a steady slow-flowing current as it winds through the steep defiles and high cliffs of the hardest gneiss and granite, which had been opened out to receive it; for even now, where the gorges are the deepest and the narrowest, and where the river must therefore have exerted its greatest power, the buildings of Roman times have been daily bathed by the stream, and not a fragment of them has been worn away " (*Siluria*, p. 497).

Turning to Britain, he says : " No one who has examined the tract of Coalbrookdale will contend that the deep gorge in which the Severn flows at that place has been eaten by the agency of the river (there so powerless), the more so when that great fissure in the Silurian rocks is at once accounted for by their abrupt severance, with an entire un-conformity between the strata of Wenlock limestone occupying the opposite sides of the valley. Now, in that part of Shropshire the Severn has not worn away the slightest portion of the rocks during the historic era, nor has it scooped out a deeper channel; it has only deposited silt and mud and increased the extent of land upon its banks."

Mackintosh, a keen observer, says : " In the cliff known as High Tor at Matlock there is a rent. . . . The neighbouring configuration of the ground shows that a fresh-water stream could not have flowed through this fissure without first running uphill " (Mackintosh, *Scenery*, etc., pp. 177, 178). In regard to the fluviatile theory of the erosion of the Cheddar gorge, the same writer again says : " Such a river must have flowed uphill before it commenced acting on the summit of the limestone ridge, unless we assign to rain a power of having washed out the open area behind while the defile was in course of being excavated—an assumption at variance with the fact that a great part of the open area consists of rock

similar to that forming the sides of the defile. A subaerial stream could never have scooped out the caves which at various levels open abruptly into the defile at nearly right angles to its course. Neither could it have assumed so peculiarly tortuous a course as that of the Cheddar defile while it preserved sufficient inclination of channel to enable it to excavate, unless, indeed, it followed the windings of a rent, in which case it would not have acted upwardly so as to strip off the great mass of strata on the dipside of the defile, in many places conformably to the angle of dip. But supposing a subaerial stream to be capable of all this while it flowed along a steeply inclined channel, it must have lost the necessary degree of transporting and grinding power long before it reached so low an angle of inclination as that of many parts of the bottom of the Cheddar defile " (Mackintosh, *op. cit.*, pp. 138, 139).

Ruskin has given a graphic and, as it seems to me, an admirable answer to the champions of river and lake erosion by running water, and takes as his example the valley of Yewdale in the English Lake District. Of this he writes: " With what chisel has this hollow been hewn for us? Of course, the geologist replies, by the frost and the rain and the decomposition of its rocks. Good; but though frost may break up and the rain wash down, there must have been somebody to cart away the rubbish or still you would have no Yewdale. Well, of course, again the geologist answers, the streamlets are the carters, and this stream past Mr. Bowness's smithy is carter-in-chief.

" How many cartloads, then, may we suppose the stream has carried past Mr. Bowness's before it carted away all Yewdale to this extent, and cut out all the northern side of Wetherlam, and all that precipice of Yewdale Crag, and carted all the rubbish first into Coniston Lake and then out of it again, and so down the Crake into the sea? Oh, the geologists reply, we don't mean that the little Crake did all that. Of course it was a great river a quarter of a mile long, or it was a glacier five miles thick going ten miles an hour, or a sea fifty miles deep, or something of that sort. I want to know here at the side of my little puzzler of a pool whether there is any subaerial denudation going on still, and

whether this visible Crake, though it can only do little, does anything ? Is it carrying stones at all now past Mr. Bowness's ? Of course, reply the geologists, don't you see the stones all along it, and doesn't it bring down more every flood ? Well, yes; the delta of Coniston Waterhead may perhaps within the memory of the oldest inhabitant or within the last hundred years have advanced a couple of yards or so. At that rate those two streams, considered as navvies, are proceeding with the works in hand—to that extent they are indeed filling up the lake, and to that extent subaerially denuding the mountains. . . . The streams, we say, by little and little are filling up the lake. They did not cut out the basin of that. Something else must have cut out that, then, before the streams began their work. Could the lake, then, have been cut out all by itself and none of the valleys that lead to it ? Was it punched into the mass of elevated ground like a long grave before the streams were set to work to cut Yewdale down to it ?

" You don't for a moment imagine that ? Well, then, the lake and the dales that descend with it must have been cut out together; but if the lake not by the streamlets, then the dales not by the streamlets. The streamlets are the consequence of the dales, then, not the causes; and the subaerial denudation to which you owe your beautiful lake scenery must have been something not only different from what is going on now, but in one half of it at least contrary to what is going on now. Then the lakes which are now being filled up were being cut down, and as probably the mountains now being cut down were being cast up.

" Don't let us go too fast, however. The streamlets are now, we perceive, filling up the big lake, but are they not then also filling up the little ones ? If they don't cut Coniston Water deeper, do you think they are cutting Mr. Marshall's tarns deeper ? If not Mr. Marshall's tarns deeper, are they cutting their own little pools deeper ? This pool by which we are standing, we have seen it is inconceivable that it should be cut deeper down. You can't suppose that the same stream which is filling up the Coniston Lake below Mr. Bowness's is cutting out another Coniston Lake above Mr. Bowness's. The truth is that above the bridge as below

it and from their sources to the sea the streamlets have the same function, and are filling not deepening alike lake, tarn, pool, channel and valley. . . . We must look, then, for some other chisel than the streamlet" (*Deucalion*, i., pp. 217-221).

" Any of you," again says Ruskin in his usual graphic way, " who have fished the pools of a Scottish or a Welsh stream, have you ever thought of asking an old keeper how much deeper they had got to be while his hairs were silvering? Do you suppose he would not laugh in your face? Any of you who have fished in the Dove or Derwent, in the Tweed or Teviot, can any of you tell me a single pool, even in the limestone or sandstone, where you could spear a salmon years ago and can't reach one now? Do you know so much as a single rivulet of clear water which has cut away a visible half inch of Highland rock, to your own knowledge, in your own day? . . . Yonder little rifted well in the native whinstone by the sheepfold, did the grey shepherd not put his lips to the same ledge of it to drink when you and he were boys together? Does any Scotsman know a change in the Fall of Foyers, any Yorkshireman in the Force of Tees? . . . It is true at the side of every stream you see the places in the rocks hollowed by the eddies, . . . but I simply ask: has any human being ever known a stream, in hard rock, cut its bed an inch deeper down in its hard bed? I can look back myself half a century and recognise no changes whatever in any of my old dabbling places, but that some stones are mossier and the streams usually dirtier—the Derwent above Keswick, for example " (*Deucalion*, i., pp. 35-37).

" Think what would be the result," says Ruskin again, " if any stream among our British hills at this moment *were* cutting its bed deeper. In order to do it, it must of course annually be able to remove the entire zone of débris moved down to its bed from the hills on each side of it and somewhat more.

" Take any Yorkshire or Highland stream you happen to know, for example, and think what quantity of débris must be annually moved on the hill surfaces which feed its waters. Remember that a lamb cannot skip on their slopes but it stirs with its hoofs some stone or grain of dust which will more or less roll or move downwards. And no frost can

break up without materially loosening some vast ledges of crag and innumerable minor ones, nor without causing the fall of others as vast or as innumerable. Make now some effort to conceive the quantity of rock and dust moved annually lower past any given level traced on the flanks of any considerable mountain stream over the area it drains, say, for instance, in the basin of the Ken above Kendal, or of the Wharfe above Bolton Abbey. Then if either of these streams were cutting their beds deeper, that quantity of rock and something more must be annually carried down by their force past Kendal Bridge and Bolton stepping-stones, which you will find would occasion phenomena very astonishing indeed to the good people of Kendal and Wharfedale.

"But it need not be carried down past the stepping-stones, you say, it may be deposited somewhere above. Yes, that is precisely so; and wherever it *is* deposited the bed of the stream or of some tributary streamlet is being *raised*. Nobody notices the raising of it—another stone or two among the wide shingle, a tongue of sand an inch or so broader at the burn side—who can notice that? Four or five years pass; a flood comes, and farmer so-and-so's field is covered with slimy rain, and farmer so-and-so's field is an inch higher than it was for ever more—but who notices that? The shingly stream has gone back into its bed, here and there a whiter stone or two gleams among its pebbles, but next year the water stain has darkened them like the rest and the bed is just as far below the level of the field as it was. And your careless geologist says : what a powerful stream it is and how deeply it is cutting its stream through the glen " (*Deucalion*, i., pp. 70-72).

" You might naturally think," says the same writer, following out the idea of subaerial denudation, " that the sudden and steep rise of the crag above those softer strata was the natural consequence of its greater hardness, and that in general the district was only the remains of a hard knot or kernel in the substance of the island from which the softer superincumbent or surrounding material had been more or less rubbed or washed away.

"But had that been so, one result of the process must have been certain—that the hard rocks would have resisted

more than the soft; and that in more distinct proportion and connection the hardness of a mountain would be con- jecturable from its height, and the whole surface of the district more or less manifestly composed of hard bosses or ridges, with depressions between them in softer materials. Nothing is so common, nothing so clear, as this condition on a small scale in every weathered rock. Its quartz or other hard knots and veins stand out from the depressed surface in raised walls like the divisions between the pits of Dante's eighth circle, and to a certain extent, Mr. Ward tells us, the lava dykes, either by their hardness or by their decomposition, produce walls and trenches in the existing surface of the hills. But these are on so small a scale that they cannot be dis- cernibly indicated on a map, and the quite amazing fact stands out in unqualified and indisputable decision, that by whatever force these forms of your mountains were hewn, it cut through the substance of them, as a sword-stroke through flesh, bone and marrow, and swept away the masses to be removed with as severe and indiscriminating power as one of the shot from the Devil's great guns at Shoeburyness goes through the oak and the iron of its target " (Ruskin, *Deucalion*, pp. 223, 224).

I will now turn again to Mr. John Murray, who has quoted some very good illustrations to prove the position I am arguing for.

Thus he refers very aptly to the well-known reef stretching across the Rhine at Bingen, upon which so many ladened barges have suffered wreck during hundreds of years. Notwithstand- ing that the full stream of the Rhine during so many ages has been unable to wear away this comparatively slight barrier, we are taught by geologists to believe that the long avenue of lofty precipices, including the Lurley a little lower down, and consequently the whole of the gorge from Bingen to Neuwied, sixty miles long, have been cut through by this same river Rhine. The Iron Gate on the Danube, just below the even more stupendous gorge through which that river passes out of Hungary, presents a similar obstacle to naviga- tion and to geologists who have failed to explain to us how water erosion, having, as they assert, cut through cliffs 2,000 feet high, should have stopped short at this petty barrier reef.

The same writer, speaking of the Upper Rhine, says: " Though a broad river above the pass of the Via Mala, it sinks invisible or is reduced to a mere thread at the bottom of this most remarkable fissure. But cliffs and precipices occur all over the world, and there is no distinction in form and structure between those which bound seas or river courses and those which occur inland and far away from running water. Why should the one class of cliff have a different origin from the other? The gorge of Goschenen on the St. Gothard Pass is traversed by the furious torrent Reuss, but its valley runs uninterruptedly into the Lake of Lucerne, whose precipices are loftier than those about the Devil's Bridge and as straight, though no running water washes their base but only a deep lake " (Murray, *op. cit.*, pp. 65, 66).

The valley of the Jordan is a good test case of the erosion theory. Prof. Huxley, who had made a special study of Palestine (but who was devoted to that theory), declares, with the survey of the Dead Sea by Lartet before him, that " rain and running water working along this old line of fracture ultimately hollowed out the valley of the Jordan, in fact determined the present configuration of the country " (*Nineteenth Century*, No. 1). The surface of the Dead Sea, which is a mere widening out of the Jordan, is sunk 1,300 feet below the Mediterranean, with the further obstacle of a watershed intervening in the valley of the Ghor, between it and the Red Sea, to intercept an overflow in that direction. A line of fracture in the strata forming the valley of the Jordan extends north and south for 160 miles. In the crack lies the river bed of the Jordan, and when it reaches the Dead Sea the rocks on either side not only do not correspond as they would have done had the hollow been caused by atmospheric erosion, but the chalk cliffs on the western side of the Dead Sea are totally different from the red sandstone (*grès de Nubie*) which occupies the eastern (Moab) shores, owing to the sandstone having been lifted up many hundred feet, while the cretaceous rock on the west is depressed.

It is literally incredible that Huxley, with Lartet's sections before him illustrating these facts very completely, should have given his countenance to such a theory as the one he puts forward to explain the Jordan valley. It is only a fresh

proof of the absolute detachment of the modern school of geologists from inductive methods.

Mr. J. Murray quotes, as a critical example of a river bed made *for* a river and not *by* it, the case of the Zambesi. He thus describes it : "This commanding stream, having attained a width of more than a mile, flowing from north to south along an undulating plain bounded by distant hills, on a sudden drops down into a crack stretching directly across its course, forming a trough 350 feet deep but not more than eighty feet wide, into which the whole body of water is discharged. The fall is twice as high and twice as wide as Niagara, but differs from it in that immediately opposite to the fall rise three successive natural walls of rock of the same height as that over which the river leaps, separated from each other by narrow rifts. These triple barriers consist of wedge-shaped promontories of rock with vertical sides, projecting alternately from the right bank and from the left, like side-scenes in a theatre, but entirely overlapping one another. Out of the first deep trough the river, after its descent, is compelled to find its way through a gap only eighty yards wide in the first opposing rock wall. A second wall here confronts it, by which the stream is turned at an acute angle to the right. It is next forced round the second promontory, then, reversing its course, round a third, and before it is allowed to escape to the sea it is compelled to double round a fourth wider headland.

" If the irresistible erosive power attributed to running water really existed in it, the intrusive wall thus thrusting itself in front of the cataract should have been swept away by it long ago, instead of which the hard basalt over which the river tumbles has not yet lost its sharp edge, and the floods of thousands of years have surged against the opposing precipices without the slightest apparent enlargement of the wonderful, deep, zigzag channel. The profound abyss into which the Zambesi falls is so narrow that it is difficult to discern athwart the blinding spray the vast flood at its bottom; but the surging river, however much it may chafe within its bounding walls, is turned backwards and forwards by them, to right and to left according as they direct its course."

What action or application of running water could cause

a river of first magnitude, flowing over a flat surface of rock, thus suddenly to drop into the bowels of the earth? By what operation did it make this zigzag ravine channel? Was it by cutting back? Then how came it not to sweep away these rock-partitions, so narrow in places that two men can scarcely walk abreast along them? Still more preposterous is it to suppose that such a river could reverse its current on a sudden, so as to cut sideways, first right to left, next left to right. Sir A. Geikie was evidently greatly puzzled to account for this critical case of so-called river erosion (! ! !), and he suggests in explaining it that "the river seems to have cut its way backward through the winding ravine, until, owing to some subterranean movements effecting a change of level or to some other cause, the body of water in place of entering at the top of the ravine has been emptied over its sides". To this Mr. Murray conclusively replies: "This winding ravine is in reality a series of cracks ending in narrow points. The river is not emptied over its sides. There can have been no change of level, the top of the rocks at the fall being even with the river bed above. The discovery of the Zambesi Falls," he continues, "would seem to have been reserved until the present time, in order to refute a leading tenet of modern geology, and to prove the utter impotence of water to cut through hard rock. The conclusion seems irresistible that the fissure was made for the river to pass through, possibly by some shrinkage of the basaltic rock" (*Scepticism in Geology*, pp. 68-70).

Again, he says: "We find mountains split through to allow rivers to pass in all parts of the world. If, then, water made such gorges, how was it carried up to the top of these mountains? How could it commence operations on a curved slope? How could water have rested on such an incline?"

Let us now turn to the Caucasus. Prof. H. Sjögren describes a famous gorge in that range. He says: "The gorge traverses the main chalk cliff ridge in the direction N. 40° E., then changes its line to N.W., which it still follows at the widening of the valley below Tijerkei, and finally comes back to due N., as it passes through the tertiary hills below Subut. The mean height of the ridge thus cut asunder is some 2,000

metres, or more than 6,500 feet; in the summit of Salatan, which lies about three miles from the gorge, it reaches nearly 8,000 feet; the bed of the river Sulak at its entrance below Gimri is about 1,000 feet, and at its exit near Tijerkei about 6,000 feet above the sea. The huge cutting has therefore a vertical depth of from 5,000 to 6,000 feet, while its breadth is so small that the river leaves no room for a proper road, and scarcely enough for a narrow horse-path, which is itself impossible at certain seasons of the year. The walls of the defile mainly consist of a compact dolomitic limestone and show the lines of stratification with unusual distinctness, they rise almost perpendicularly into the air and are altogether unscalable. For this reason the valley of the Sulak, which forms a hydrographic link between the two divisions of the province, is of no significance whatever. The road between Temir Char Schuro and Gimri is carried over the 6,000 feet to which the intervening range of mountains here rises instead of being taken through the river gorge. This circumstance is sufficiently indicative of the wildness and inaccessibility of this transverse valley."

It seems to me perfectly fantastic to attribute a gorge of this kind to any other action than that of a fracture, and more especially do I deem it impossible to explain the work as the author quoted does. The erosion of the small rivers must in that case have cut right across the strike of the beds, as he himself describes, through a complicated region of fundamental schists and lias, and in some places cuts obliquely through huge folds of jurassic and cretaceous craters. In order to make his fluvial notion work, Sjögren has to postulate that the bed of the Koissu rivers and the Sulak originally lay at so high a level that the water flowed over the great range of mountains without cutting it, and that the transverse valley which still exists was subsequently eroded to its present depth, its erosion keeping pace with the general denudation of the valleys which lay behind it.

The same writer describes a second gorge through which the river Gerdeman Tschaj flows. This tremendous defile is in places 3,800 feet deep and is cut partially through clay slates, half crystalline limestones, massive basalts and andesites, etc., and this has been done by a stream described

by Mr. Sjögren himself as comparatively insignificant, which in certain seasons of the year runs almost dry, and has so trifling a volume of water that it disappears completely in the Kura Steppe without reaching either river or lake of any kind; and this small volume of water, concludes the writer, "has been able to execute so grandiose a piece of work". The phrase sounds like a sarcasm. It only shows once more how near akin the sublime and ridiculous really are. To me these rents and rifts are clearly connected with the subterranean forces that fashioned the mountains of Daghestan and with the great masses of basalt and other intrusive and igneous rocks which have tossed the strata about in all directions thereabouts, setting some of them vertically on end and folding and refolding the slates and limestones as our author describes. Such clefts are, as we have seen in many cases, the necessary consequences of those subterranean strains which are so distasteful to the uniformitarian, who is continually inventing very formidable machinery to crack very simple nuts with.

The fiords of Norway, Greenland and North America, again, are assuredly as different to any products of river erosion as can be conceived. Not only are they often narrow rifts, bounded by perpendicular cliffs of crystalline rocks 1,500 to 2,000 feet high and headed by precipices, a condition of things unknown in any hollows worn by rivers, but the waterfalls which at intervals fall over their sides have not worn any lateral ravines, which precludes the notion that they may have been cut back by the waterfalls at their heads. The contour of the floors of the fiords as shown by soundings is also quite contrary to that of any river bottom, for they rise sharply upwards at thĕir termination after running far below sea level. It is only the suggestion of despair to convert such fissures into river valleys.

The most stupendous of all these phenomena, however, which have been attributed to the erosion of water, are the great cañons of Colorado. " The principal facts," says Dana, " are these : *A length of* 200 *miles, and through the whole nearly vertical walls of rock* 3,000 *to* 6,000 *feet in height. These rocks, limestone and other strata of carboniferous age, others of older palæozoic, and below them generally the solid granite, making from*

500 to 1,000 feet of the gorge, and in some places the granite rising in pinnacles out of the waters of the stream, finally, all the tributaries or lateral streams with similar profound gorges or chasms." Dana says that Newberry attributes these profound gorges, *and beyond doubt correctly, to erosion, each stream having made its own channel* (Dana's *Manual,* pp. 640, 641).

This view is endorsed by many geological authorities, but not by any means by all of them. I will quote from a sounder judge, whose views seem to me a great deal more sane. Prestwich objects, among other things, to the attribution of these cañons to river action, on the ground that " traces of the presence of the water at the successive levels are commonly left in such cases, and he says he is unaware of any such high-level river deposits having yet been pointed out. The narrowness of the passes and the perpendicularity of the walls may in many places have left no room for such lodgment, but there are more open spaces where they could have been preserved. Nor are there any old deserted river channels in which the present rivers formerly flowed at high levels. Nor do the cañons show that width on the surface which is usually the result of river action before the rivers settle into a definite channel, and it is difficult to believe that over an area so wide and with such a variety of conditions all such traces should have been entirely removed. . . . With rivers of such power as the Colorado it is scarcely probable that they would have kept within the very narrow limits of their present channels, especially during their first stages " (*op. cit.,* pp. 95-98).

This seems to me to be a moderate and sensible statement of some of the difficulties of assigning a fluviatile origin to the cañons. I shall have more to say about them and the problem involved in their explanation in Chapter IX., and now propose to turn to another part of my case, which I shall introduce by a concrete example. Mr. John Murray quotes the case of the Litany (the ancient Leontes) which, coming down from the eastern slope of Lebanon, descends the valley between it and Anti-Libanus for more than thirty miles. At that point it approaches within ten miles of the headwaters of the Jordan. A watershed of not more than fifty feet elevation, rising directly in the line of its previous course, north and south,

alone separates the two valleys. Precisely at this spot the Litany alters its course, turns abruptly at a right angle due west, in order to enter the defile of Kuweh, in places no more than ten or fifteen feet wide, which cleaves the chain of Lebanon to a depth of 600 feet at least, and through this it enters the sea a little to the north of Tyre. The Litany, if left to itself, according to the laws of hydrostatics, must have followed the lower opening presented to it, and have flowed over the inconsiderable watershed into the Jordan, and thence to the Dead Sea. It is equally clear and certain that it could not have turned round, risen up 600 feet, and cut its present bed through so lofty and rocky a chasm, when the low road was open to it, without changing the line of its previous course.

Coming nearer home, Mr. Murray asks : " Why should the Avon on quitting Bristol have altered its course, and instead of running straight forward over the low ridge at Bedminster into the Bristol Channel have turned north to encounter hills five times higher (400 to 500 feet) than those of Leigh Downs, unless it had found the gorge of Clifton opened ready for it ? That that gorge was produced by a great convulsion is undeniable from the remarkable fault a little below the Suspension Bridge, by which the strata on one side have suffered a vertical displacement of 800 feet above those of the other. In both these cases we may fairly ask the erosionists what could possibly have induced *rivers to run uphill*, to surmount ridges many hundred feet high, and then to saw through mountains many miles thick, when a clear low gap was offered to them with the least possible amount of erosion."

The phenomenon presented in the last quoted instance, which is a very frequent one, and which necessitates our believing, if we accept the original erosionist theory of the origin of valleys, that rivers will deliberately turn aside from their natural, simple march along the routes prescribed for them by ordinary dynamical laws and the laws of gravitation, and proceed to carve great clefts and fissures for themselves through very hard rocks at right angles to their course or even at sharper angles, has always been a difficulty for the champions of this theory. It is so palpably against the teaching of physics and against every natural probability.

The flow of water is a simple hydrodynamical problem. It simply follows the laws of gravitation and makes for itself the easiest passage it can. When it has a perfectly plain and simple road before it, however long and however meandering, it follows it and does not attempt, to use a homely phrase, to run its head against a stone wall, and a stone wall, too, which is not in its direct course but which it has to turn aside to butt against.

If, then, we find that in a large number of cases rivers do make an elbow-turn in their journey instead of travelling straight ahead, and thus pass through a fissure or gap in a mountain range instead of along its flanks, simple people (who have not drunk at the fountain of wisdom where the metaphysical and not the scientific doctrine of uniformity is taught) suppose, and very reasonably, that the reason a river does this is because it finds the road open and takes it, it being the shortest and easiest path. To that simple creed I most completely adhere, and I protest against the view that under any system of hydrodynamics known to us water could act in any other way, and could deliberately turn aside, as it is supposed to have done, and force itself a road through what often are hard crystalline rocks. *En passant* I would remark that if it were in the habit of doing so, it is very strange that we should never come across examples of the process actually incomplete and have an opportunity of seeing the boring process when half done. The question of these rivers turning aside in this fashion is only a particular instance of the crux involved in the transverse valleys, which cut across the limiting mountains of much longer longitudinal valleys in many places; of, in fact, a more general law, by which long valleys lying between ranges of hills often have subsidiary valleys which are known as transverse valleys, running at right angles to their own course. How to account for such a state of things by any theory of fluviatile erosion is indeed a difficulty.

Conybeare discussed the question long ago when it was thought that both sets of valleys were eroded at the same time by rivers from the same watershed. He says : "It is quite obvious that since these longitudinal valleys have existed the waters could never have risen within several hundred feet of

the summit of the chains over which, on the fluvial hypothesis, they are supposed to have flowed. It must be argued, then, that at first no such longitudinal valleys existed . . . but the whole surface at first presented one uniform declivity nearly uninterrupted. The fluvialists must then suppose that the drainage across this declivity excavated the transverse valleys as its main channels, while the lateral drainage into these main channels excavated the longitudinal valleys. But in order to constitute the supposed original uniform declivity, the mass of materials formerly upfilling the whole space, and which we must imagine to have been subsequently removed, was stupendously great, for these longitudinal valleys usually present very extensive plains at the foot of the basset escarpments, whereas the transverse valleys are comparatively narrow defiles. If, then, we attribute the latter to the main course of drainages, and the former to its lateral action, we attribute an inferior effect to what must surely be considered as the most favourable line of action, and a vastly superior effect to that least favourable " (*Phil. Mag.*, ix., pp. 259, 260).

Again, speaking of the great transverse breach between the chalk wolds of Lincolnshire and Yorkshire giving vent to the outlet of the Humber, he says : " All the flats near the junction of the Trent and Derwent would have formed an immense lake, whose waters would have been so dammed up as to have flooded all the lower portions of the Ouse and Swale, and discharged themselves finally by the mouth of the Tees, as the escarpments of the chalk wolds, and afterwards of the eastern moorlands, would have presented an insuperable barrier, preventing any other egress to the sea basin excepting Teesdale, previously to their fracture by transverse valleys. Now, in order to get over this difficulty, the fluvialist must, I conceive, argue that at the time when these streams commenced their operations the said escarpments presented no barriers at all, all the valleys on the west of them having been at that period filled up (by materials since removed) to such a level as to overtop the chalk and oolitic ranges ; since by such a configuration of surface alone would the streams have been brought to act on these ranges so as to cut transversely through them. Let the fluvialist, however, so reconstruct the district in question. I next ask what it will require to

reduce it from this, 'its form ten million years ago,' to its actual features. Why, simply the excavation of the entire vales of Lincoln and York (a district about 100 miles long and more than fifteen broad) to a depth of 700 feet beneath its supposed original level. I would ask, how long would atmospherical drainage take to effect this? Seeing that since the Romans occupied Eboracum, 1,700 years ago, that agency has not effected a degradation of seven inches on any one of the valla of their encampments, I leave the fluvialists to work out the question at leisure, offering in the meanwhile, as a mere approximation, an infinitellion of ages in the nth power."

Let me add some more general remarks of Conybeare's on the appeal to river erosion of the Weald. He says : " Most of the main streams have their headwaters in the central axis; whence those running northwards into the Thames have to intersect by transverse valleys the two barriers of the Kentish rag hills and of the northern chalk downs, neglecting the two intervening longitudinal valleys into which a dam of less than 100 feet high erected in any of these breaches, which are about 600 feet high, would turn the drainages towards the Straits of Dover. Such are the circumstances of the Wey, the Mole and the Medway; the Darent and the Stour rising almost within the limits of the rag hills, indeed, can scarcely be said to break through more than one of these barriers, the chalk. On the south side we have the Arun, the Adur, the Ouse and the Cuckmere, which in like manner break through the single opposing barrier of the chalky South Downs (as the sands do not on this side present a regular escarpment). It is quite inconceivable that fluviatile erosion could have produced such a configuration, unless we suppose that the surface originally, when the drainage commenced its work, presented uniform slopes from the central axis to the estuary of the Thames on the one side, and the sea on the other, the intermediate longitudinal valleys having been then filled up; and that while the direct drainage was excavating the transverse valleys, the lateral drainage excavated the longitudinal valleys. Why has the lateral drainage produced so much more considerable effect than the direct drainage? Secondly, how has it happened that the lateral drainage into so many distinct

main channels has coincided so as to form one uniform longitudinal valley, instead of ramifications extending from one principal stream without any relation to those of the next principal stream ? While the geologist is studying the valleys, the antiquary will observe throughout this tract the boldest prominences of the escarpments studded with ancient earthworks, which, though placed in the most exposed situations, have resisted the action of atmospherical causes for some twenty centuries " (*ibid.*, pp. 261-263).

The difficulties here pointed out by Conybeare are only some of those which present themselves when we try to realise why rivers with perfectly open ways to the sea should choose to turn aside and force their way by very long and desperate methods of erosion through chains of mountains. The difficulties in question have been a perpetual trouble to the fluviatile erosionists.

It was, therefore, a great godsend to them when Mr. Beete Jukes, who was then at the head of the Irish Survey, following in the wake of Playfair's " longitudinal valley and transverse outlet " theory, and of Colonel Greenwood, the great champion of meteoric origin, the author of *Rain and Rivers*, who had urged the differentiating tendencies in regard to erosion of hard and soft rocks when in close proximity, propounded his now famous theory.

Jukes had been committed for years to the notion that rivers erode their valleys. In order to meet the difficulty of the transverse valleys he invented one of the most extraordinary hypotheses that ever entered the mind of man. This he published in an article on the river valleys of the south of Ireland in the *Quarterly Journal of the Geological Society of London*, vol. xviii.

The solution he there published, he tells us, requires that the rivers should never have ceased to run through the ravines, by which they traverse isolated hills between their sources and the sea, during the denudation of the plains by which those hills are surrounded. . . . " The reason why the rivers choose to run through the hills by deep ravines, instead of by much easier routes which are now open to them, is that when they began to run these hills did not exist. The hills were then buried as it were in much higher ground, by which

they were surrounded, and over which the rivers originally ran. The rivers choosing, of course, the lowest ground they could find in their course to the sea, happened here and there to cross the parts where these hills subsequently became disclosed by the waste and erosion of the rock which surrounded them. The rivers, however, having once cut channels for themselves, have ever since kept these channels open, and it is through these channels that the waste of the interior has been carried off. Although, then, the interior was worn down into a plain, while the hill ground resisted that action and was left as a hill, the river channel, through that hill, was always cut lower than any part of the plain, for it was only in consequence of the deepening of that channel that the waste could be carried off and the erosion of the surface of the plain continued.

"In Ireland the rock that was thus wasted in the interior was carboniferous limestone, the ground that stood as a hill was old red sandstone or some other silicious rock.

"The calcareous rock was acted upon both by mechanical erosion and chemical solution, the silicious rock only by mechanical erosion. The silicious rock, therefore, resisted the atmospheric action far more than the calcareous rock did, but it would not thus have resisted the sea, which would have cut into old red sandstone just as easily as into carboniferous limestone." Jukes says this explanation came upon him like a revelation (*Geol. Mag.*, iii., pp. 232, 233). To most other people it would seem to have been a very bad dream. What a number of extravagant postulates it involves, and all to explain what is so simple when we remember that when hog-backed mountains are raised up transverse fissures are necessarily produced in them by fracture.

First we have to postulate that the rocks supposed to have been denuded were ever there at all, which is itself a vast postulate. It proved to be a critical difficulty, and Jukes was compelled to admit it, although, like a good many parents, he did not therefore abandon his offspring. It was shown, in fact, that the softer carboniferous rocks which, he argued, once occupied the synclinal hollow between the two bounding anticlinals in the Irish district he described, and which, in his view, were worn away, never existed there at all, and

that if any rocks were ever there it was not carboniferous
limestones subject both to chemical and mechanical erosion,
but a series of hard grits known as the Coomhola grits and
carboniferous slates, so that the postulated contents of the
trough could not have been dissolved by a double process of
dissolution.

This was an awkward fact. It was not the only one Jukes
had to face. He was reminded by Mr. Mackintosh that in
South Herefordshire and Monmouthshire the conditions are
exactly the reverse of those he had postulated in Ireland.
There it is the mountain limestone which must have formed
the resisting rock, while it is the sandstone which has been
worn away. To this Jukes replied that the old red sandstone
of Herefordshire is so soft as to be an exception to the general
rule as to limestone being the yielding rock, but, as Mackin-
tosh says, the narrow channel of the Wye has been excavated
in the sandstone as well as the limestone.

This is the case also with the Avon at Clifton. There
Jukes postulated that the longitudinal valley was protected
from dissolution by a capping of newer rocks, an argument
which fails at once when applied to such a case as the Vale
of Winscombe. As Mackintosh shows, this vale contains
dolomitic trias resting on mountain limestone and old red
sandstone, and must have been scooped out of the latter
rocks before the triassic period. What, then, was there to
protect the now surrounding limestone ridges from subaerial
decay while the vale was in course of being denuded out of
the nucleus of the Mendips, and while, according to Jukes'
theory, the gorge between Crook's Peak and Hutton Hill,
through which the drainage now escapes, was in course of
being worn down through the limestone? (Mackintosh,
Scenery of England, pp. 227, 228.)

If we try the theory by such cases as the Weald Valley we
shall see how futile it is. There uniform chalk must have
covered both anticlinals and synclinals. Jukes, with the
courage which is always available to the champions of uni-
formity, accordingly postulated a previous and portentous
and, as we shall see, a quite incredible marine erosion to
remove the chalk so as to enable the problem to be solved
according to his premises.

Not only is there, as we shall see, no reliable evidence of any such marine erosion in the Weald area, but the contour of the surface of the Weald beds, which form a raised hogsback in the middle of the synclinal valley, is utterly opposed to any such explanation and a clear proof that there has been an upheaval of the forest ridge.

Again, it is not easy to see how any river could exist at all in the longitudinal valley running along the Weald depression if its bounding ridges were pierced at intervals on either side by transverse valleys which already existed when the longitudinal valley began to be formed. These transverse valleys must have drained off every particle of water running across their mouths, and the only denuding agencies left would be merely meteoric ones, the efficiency of which I have already discussed.

If the transverse valleys were cut before the intervening chalk between the Downs was destroyed, whence could the necessary streams for denuding purposes be obtained? Where do we now find chalk districts supplying rivers of this kind? Chalk is singularly absorbent of water and swallows up the rain without allowing it to collect into rills, and many of the transverse valleys in the Downs neither have at present, nor ever had, streams flowing through them.

These are some only of the difficulties which present themselves when we try to elicit any meaning from this most fantastic of all hypotheses. Yet it has been taken over apparently without any inquiry by a crowd of erosionist advocates. Among these have been some very influential ones. Thus the theory was adopted by the joint authors, already named, of the *Manual of Indian Geology* to explain the mysteries of the Himalayan river valleys. They affirm that the features of the drainage of that mighty range were marked out long before the Himalayas attained anything like their present altitude, and that the rivers were able to maintain their courses transverse to the range by erosion of their beds *pari passu* with its elevation. When it was pointed out that the catchment basins of these rivers north of the Snowy Range were too small to account for such a process, an equally daring hypothesis was suggested to bolster up the cause, and it was further suggested that the transverse rivers had once been

longer, but that their courses had been shortened by a gradual encroachment of the longitudinal valleys, which, cutting back along the strike of the rocks, were able to divert the drainage into their courses. Mr. Oldham argues against this latter view that, " as the gradient of these rivers, owing to their longer course, is lower than those of those rivers which have a more direct course to the southern margin of the range, it is difficult to see how they could erode their beds *pari passu* in this fashion unless their gentler gradient were compensated by the existence of a zone of rocks much softer than are found to the south. Along part of the Indus valley in Ladakh such a band of soft tertiary rock does exist ; but there does not seem to be any reason for supposing that there is such a general pre-dominance of soft rocks along the whole course of the valleys, and it is difficult to see how the levels of these rivers, when they ultimately cross the axis of the range, could have been kept sufficiently lower than those of the other valley to enable them to encroach and rob the drainage of the latter " (*Journ. of the Manch. Geog. Soc.*, 1894, pp. 5, 6).

Baron Richthofen's theory is even more audacious. He holds that the whole regions north of the high peaks of the Himalayas was once higher even than they are now, and that the line of greatest elevation originally corresponded with the present watershed, that the present drainage system then originated, and that the present elevation of the peaks is due to their greater power of resisting denudation while the softer rocks to the north were worn down (*Führer für Forschungs-wissen*, p. 175).

All this is surely building houses out of cloudland, and is not science. The notion is in fact a purely imaginative one and based upon no direct evidence, and only upon the necessities of an *a priori* argument. To return to Europe, however. The notion that the Weald was originally stripped of its chalk covering by marine action, and then worn down and denuded by meteoric agencies and by rivers, has been re-vived in another way by Messrs. Foster and Topley in a well-known memoir published in the *Quar. Journ. Geol. Soc.* Their views have been very largely adopted. In regard to the initial step in their argument, namely, the marine erosion, I shall have more to say presently. The subsequent meteoric

and fluviatile erosion of the beds subjacent to the chalk has been maintained by the two authors just named, so far as I can see, entirely on the ground that gravels which they attribute to the Medway in old days are found at a height of 300 feet above the present level of that river. On this basis alone has been erected the astounding postulate that the Medway once flowed 300 feet higher than now, and that since the Medway ran at the 300 feet level at East Malling, all the Weald area has been denuded. When we add, they say, " that a large part of this area is 200 and even 250 feet below the gravel at East Malling the vast amount of the denudation will be perceived; and all this denudation has been due to the action of rain and rivers, for we have shown the Medway deepened its valley gradually, and not only are there no traces of marine action, but had the sea had access since the gravel was deposited, surely it would have swept away such loose and incoherent materials. If rain and rivers could do so much, if they could cut out a valley 250 feet deep and seven miles broad, surely we may allow that by giving them more time they could scoop out valleys 500 feet deep; in other words, that making every allowance for slight superficial inequalities produced by marine denudation, all existing inequalities in the basin of the Medway, including the greensand escarpment and the chalk escarpment, are entirely due to atmospheric denudation, that is to say, to the action of rain and rivers. If this holds good for the basin of the Medway, it may be applied to the whole of the Wealden area " (*op. cit.*, xxi., pp. 464, 465).

Here, then, we have a portentous deduction all based on a single fact, namely, the occurrence of certain gravels at 300 feet above the Medway at Malling. These gravels can be explained, and in my view can be alone explained by a very different theory, and no more prove the former presence of the Medway where they are found, than the plateau gravels mantling wide stretches of country, irrespective of its contours, prove fluviatile action there. This I shall revert to, however, in a later chapter. At present I am chiefly exercised to point out one or two critical difficulties in this solution of the problem. In regard to the rivers which they invoke, our authors acknowledge that they have left no trace of their

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handiwork, but they say: "The reason why we have no traces of river action at the higher levels is that in the long lapse *of time these old alluvia have themselves been removed by subaerial denudation*". What an extraordinary argument we have here. Who ever heard of banks of gravel, especially of flint-gravel, etc., the most lasting and most conservative of monuments, and especially in a valley well clothed with vegetation and increasing instead of diminishing its surface beds, being all so completely swept away by subaerial denudation as not to have left a trace behind.

Searles Wood (*Geol. Mag.*, iii., p. 404) further points out that the gravels referred to by Topley and Foster cannot have all been brought down by the Medway flowing as it does now, for the greater part of the gravel, in the greensand terrace, came from the north, and was therefore foreign to the area, and was not due to denudation *in situ*.

Let us proceed, however. In regard to those high valleys, known as mountain passes, which by Greenwood and others have been treated as the result of fluviatile denudation, Mackintosh points out that in many, if not all, the passes in England and Wales the sides rise higher above the floor of the pass in the middle than towards each end. " To assume, then, that the two streams (which Colonel Greenwood says point their fingers at each other across the summit area, wearing their sources backwards) could have left the culminating level of such a pass, would be to assert that the streams achieved a maximum result where their action was at a *minimum, a specimen of reasoning which in any department of science, excepting geology, would not be considered worth a serious refutation.*" To which I cordially say Amen.

We have not yet done, however. If rivers now flowing through valleys have excavated them all, what is to be said of the erosion by postulated river action in the case of so-called dry valleys. A large number of valleys which traverse limestone and chalk districts have no rivers flowing through them, and never could have had, for their spongy and pervious rocks allow all the water in them to filter away. This fact has been remarked by several distinguished writers, and has been singularly ignored by the champions of the river erosion of valleys and lakes.

On this subject Conybeare says : " The Chilterns, like other chalky districts, abound in dry valleys which agree in structure and other features with those containing water-courses, and have been obviously excavated by the same causes, which in this case it is self-evident could not have been river waters."

Again, Mr. C. Brown, writing to the *Geological Magazine*, said that he lived " in the Cotswolds, where the valleys consist of depressions in the oolitic beds and have a basement of clean oolitic gravel with the edges taken off but not formed into pebbles, proving that it has never been subjected to long continued attrition. . . . Some of these valleys begin at the crest of the oolitic range, now elevated 1,000 feet above the sea, and gradually descend the south-eastern slope of the Cotswolds until they reach the summit level of the Thames, 400 feet above the sea; others are more local, descending from ground 500 to 600 feet above the sea.

" It is clear that the dry valleys cannot owe their origin to river action, and the river valleys are only channels which receive the springs of the Fuller's earth or local clay beds. The action of these rivers is never a denuding one, even when in flood, little solid matter being carried off. It is therefore impossible to conceive that these extensive valleys are the result of river action " (*Geol. Mag.*, iv., p. 139).

" As connected," says De la Bêche, " with the subject, the valleys of white limestone (in Jamaica) completely oppose themselves to the theory that valleys have been formed by the waters now flowing in them, for no waters flow in the greater part of those hollowed out of this formation, the limestone being extremely cavernous, and the rains that fall being speedily swallowed up in it ; yet these valleys are, as to form, like most other valleys, and probably owe their origin to the same causes which have produced the greater part of those in the chalk, oolite, etc., of England at the period when the diluvial gravel was formed " (*Geol. Trans.*, ii., p. 185).

Again he says : " Depressions on the earth's surface existing when the present order of things commenced, would become channels of drainage to rain water accumulating into streams and rivers. There are, however, depressions in which not even a rivulet at present flows, and of these we have examples in the white limestone districts of Jamaica, where

the inhabitants are compelled to obtain water exclusively by collecting the rain in tanks ; yet in these districts the natural inequalities of the land present the same forms of hill and dale as occur elsewhere, and even the violent rains in this tropical climate form no continuous rivers, but are swallowed by numerous sink holes or natural cavities that pervade the white limestones of Jamaica. One great valley is remarkable ; it is situated between the Carpenter and Santa Cruz Mountains, and is excavated in a white limestone interstratified with a red sandstone. . . . There is neither river nor rivulet throughout its whole extent. The river that rises suddenly near the sea, and flows but a short distance at the lower termination of this long and wide valley, is most probably derived, like many similar Jamaica streams, from waters swallowed by such holes in the interior of the islands " (*ibid.,* pp. 243, 244).

Similarly Greenough long ago pointed the same moral in the cases of the Cheddar valley, of Rocks near Linton and the Wingate near Castleton in Derbyshire. Conybeare similarly argues from the gorge of Clifton near Bristol, through which the Avon flows, as a case in point. " If this were dried the resulting lake would be drained in the direction of Nailsea and exert no action on the rocks of Clifton. The carboniferous limestone districts of England abound in examples of the second kind, *viz.,* of gorges entirely dry, or through which the rills now passing are much too insignificant to have caused them "(p. 248). I do not know what answer can be given to this.

Again, as Greenough says, " If valleys were formed by the rivers flowing through them, why is it that in so many cases the source of the river is below the head of the valley ? " This is a very common occurrence.

Let us now turn to another difficulty. De Lamétherie long ago asked the pertinent question, and it has often been repeated since his day, " If the valleys have been cut out by the streams in them, where are the débris ? Where are the chips of this gigantic stonemason's work ? " " Let us take, for instance," he says, " the valley formed by the Adriatic between the Apennines on the one hand and the mountains of Dalmatia on the other. If this valley has been carved

out by some current, where is the material gone to that once filled the present void?" He goes on to say that the Atlantic itself is only a valley on a gigantic scale, and asks the same question in regard to it.

Notwithstanding his positive assurances on the subject, Playfair himself was too keen a logician not to realise the difficulty of explaining how it is that if rivers have excavated the big valleys it comes about that the lakes which they pass through have not been filled up and choked with débris.

Speaking of the Rhône valley, he says: " If this valley, or even a large proportion of it, had been excavated by the Rhône itself, the lake ought to have been entirely filled up, because the materials brought down by the river seem to be much greater than the lake, on any reasonable supposition concerning its original magnitude, can possibly have received. What, then, it may be said, has become of all that the Rhône has brought down and deposited in it? The lake at this moment retains in some places the depth of more than 1,000 feet; and yet, of all that the Rhône carries into it, nothing but the pure water issues." Here is no doubt a great crux, and Playfair frankly confesses that he cannot entirely remove the difficulty.

If valleys were formed by erosion of rivers, the lakes through which these rivers flow must have long since been filled up by the materials brought into them. To say that the lakes were once deeper than at present is giving up the theory, for lakes are only the deeper parts of valleys.

" Had the valley of Borrowdale, in Cumberland, been excavated by the water that flows from it, the lake of Keswick, at its entrance, must have received all the materials and been long since choked up. Or had the valley of the Rhône, 10,000 feet deep and sixty miles in length, been excavated by the Rhône, the quantity of matter brought down by this river would not only have filled the lake of Geneva, into which it empties itself, but the broad valley in which the lake lies must also have been filled up and raised to the height of the Jura. That the lake of Geneva and all lakes into which large rivers flow are *gradually* filling up is true, but the valley of the Rhône is not, nor are other valleys, becoming deeper " (Bakewell, *Introduction to Geology*, p. 583).

Brongniart also asks where the débris has gone to if the

rivers have excavated the valleys. It cannot be to the sea, for the valleys are often more than a hundred leagues from the sea, and when rivers reach the plains they deposit and do not erode. They do so still more when traversing lakes. This is notably the case with Alpine rivers. Thus the Rhône crosses the lake of Geneva, the Aar the lakes of Brienz and Thun, the Reuss the lake of the Four Forest Cantons, the Linth the lake of Zürich, the Rhine the lake of Constance, the Ticino lake Maggiore, the Adda the lake of Como, the Oglio the lake of Iseo, the Mincio the lake of Garda, etc. These lakes are only deeper parts of valleys, and must have been filled up. It is abundantly clear, in fact, that the same cause which made the lake made the valley also.

Mr. J. Murray puts the same problem neatly. " What," he asks, " has become of the chips and fragments which the subaerial hammer and chisel could not fail to have left behind while carving Mont Blanc and its ten miles' array of peaks and precipices out of the solid matrix, as supposed by ortho- dox followers of Hutton ? Not only is the valley of Chamouni, measuring from the top of Mont Blanc to that of the back of la Flégère, a width of five or six miles, entirely open, but its bottom bears no trace of any such encumbrance. The river Arve rolls over a bed of rock, covered with a thin layer of boulders and gravel, the product of ancient and modern glacier moraines."

If we turn to our own island the problem, as put by the erosionists, is equally insoluble. Where, for instance, is the débris which has come from the excavation of the valleys, and notably those enormous valleys like the Weald, where the postulated denudation has been so great. The Weald valley is forty miles wide in places. Where is all the chalk ? where are the flints ? where are the vast deltas at the mouth not only of the main valley, but of the transverse valleys ?

We have said something of the filling up of lakes. This reminds us that their excavation has also been attributed to fluviatile action by some inquirers. This is very plausible, for if we appeal to rivers as the excavators of valleys it is difficult to exclude lakes. We can hardly doubt that they owe their origin to the same cause which made the valleys. Lakes are merely deeper sections of rivers, and many rivers flow through a series of lakes like a stream traverses its

salmon pools. Yet how, by any dynamical theory, are we to attribute to water the excavation of these lakes. I know not, nor have I seen any rational theory that would meet the case. Whether loaded or unloaded, how is water running, for instance, like the upper Rhône to dig out the lake of Geneva? The bottoms of deep lakes are perfectly still. There are neither waves nor currents there, and water, in order to act as an excavator of hollows hundreds of feet deep, must be able not only to run uphill, but to *work* uphill as well. I know of no lake which is now deepening itself. In order that a river may erode, it must have a passage through and not be caught in a trap or pit or enclave.

Playfair, who believed in the excavation of lakes by rivers, had still to explain the great depths of such lakes as Geneva. He says, very truly, that no mud or gravel could be carried beyond the gulf of 1,000 feet deep which was here ready to receive it, and he had no better explanation to offer than that the lake may have once consisted of some soluble deposit, like salt, which has been washed out by the water. This is assuredly a very transcendental appeal, and well may this great champion of uniformity describe it as hypothetical, and, to use his own words, " proposed in a case where the cause as visible to man seems inadequate to the effect, and where we must therefore have recourse to an agency that is invisible "— assuredly no bad concession for the arch-prophet of uniformity.

Studer said long ago that a certain incline is necessary to enable rivers even with a muddy bottom to hollow out their beds, and the slope does not exist in the lakes of the Jura or in those of Zürich and Constance. If only a great mass of water were required, he asks pertinently why the Nile and Ganges do not thus hollow out basins. Even if the slope be granted and the mass of water and the soil be favourable to erosion, we only get deep holes or *marmites,* and their extent never exceeds the radius of the direct action of the impact of the water and of the pebbles which it sets in motion. How could the Rhine at such a distance from its source have had sufficient force to hollow out a basin like that of Constance? We might on the same principle attribute the hollowing of the Caspian to the Volga and of the Dead Sea to the Jordan, and if we assume that the basin of the lake of Constance

once extended as far as the Schollberg, near Sargans, and that it was subsequently filled up by detritus as far as Rheineck, how can we suppose that the same river which in the first place hollowed out its bed from the Schollberg to Schaffhausen at more than a thousand feet below the present surface of the soil should have afterwards filled it up? (*Bibl. Univ.*, 1864, p. 101).

Sir A. Geikie, who is generally a very faithful follower of Hutton and Playfair, is obliged to confess that lakes (which are merely incidents in the life of our valleys) could not have been excavated by rivers. " It is evident," he says, " that though running water may hollow out a valley or a system of valleys, it cannot be capable of excavating a series of deep and wide cavities in solid rock " (*Glas. Geol. Soc.*, iii., p. 178). I quite agree with the latter conclusion, but I hold that one process is just as difficult to believe in as the other.

Turning from lakes, let us now consider Alpine amphitheatres, cirques, combes, corries and couses, etc. Cirques and combes, etc., are semi-circular recesses at the heads of valleys, walled all round by steep precipitous cliffs. Speaking of Hawaii, Dana thus describes their features: " The valleys lead up to amphitheatres bounded by precipices of 2,000 or 3,000 feet. . . . The traveller ascending one of the valleys along the bed of the stream finds himself at last at the base of inaccessible heights, with numberless cascades before him and a range of buttressed walls of remarkable grandeur " (*Manual*, p. 637). These cirques often contain sheets of water. There has been a great polemic as to whether they were formed by ice or not, while some able writers have attributed them to the action of water. To me both ice and water seem as incapable of scooping out anything of the kind as well may be. Let me quote from a shrewd analysis of the case in favour of their being the results of water erosion.

Mackintosh says: " To be able to judge of the effect of water in making cooms we must see what it is now doing to surfaces similar to those in which cooms have been excavated. In cooms rain is not tending to develop their typical or characteristic form. To be a coom a hollow must approximately be curvilinear. Rain is doing all it can to destroy this curvilinearity. Rain streamlets in cooms to which the ground

inclines are gullying their brims, and in all cooms channelling their sides. A continuance of the process would render a coom a mere confluence of ravines. Rain (and I may add streams) cannot keep up a uniform abrasion of the sides of cooms so as to preserve their curvilinearity. The springs which sometimes rise in cooms are the effects and not the causes of those hollows.

"In regard to the action of a single stream on an escarpment or gradual slope it produces a ravine narrowing towards the bottom and cannot produce a coom, nor can several streams combined do so."

In regard to the Glaslyn coom in Snowdon and its surroundings Mackintosh says : "It seems equally obvious that neither the present brook, nor any former representative, could have given rise to a series of phenomena so mutually opposed in form as cooms, cliffs and plateaux. What is the stream now doing in the upper part of its course, for instance under Glaslyn ? Merely rutting a continuous face of rock. What is it doing lower down ? Alternately rutting transversely flat faces of rocks, channelling marine drifts, or finding its way through peat bogs. What is it doing where all its force is concentrated under the most favourable conditions for displaying its excavating power on the steep slope of Lower Coom Dyli, for instance ? Making a maximum rut in ground which does not generally slope towards it, but presents a contour which bears no relation whatever to the channel of the brook " (*Scenery of England and Wales*, pp. 329, 330).

"After passing the sharply serrated headland of the Saddle (Cyfrwy)," says the same writer, "one of the most amphitheatrical cooms in Great Britain comes suddenly into view. The inner concave precipice presents a succession of outcropping beds, consisting of columnar felspathic trap or porphyry, slate, greenstone and felspathic ashes, surmounted at the apex by greenstone. The lower part of the coom and the whole of the basin-shaped part has been excavated out of hard felspathic trap or porphyry. Here, then, no theory founded on a supposed process of undermining caused by the wasting back of soft strata can apply " (*ibid.*, p. 149).

" Cooms," says the same writer, " generally show a tendency

to approach as near as possible to the lines which separate watersheds, and even cut through these lines or recede beyond the axes of the ridges, as in the case of the Malvern cooms, Glaslynin, Snowdonia, Coom Lisfar, etc. They therefore occupy zones where the action of running water is at *its minimum,* which seems to negative the fluvial origin of cooms " (*ibid.,* p. 190).

" That streams have not excavated cooms may be inferred again from the fact that after leaving cooms with increased volume they run down transversely level slopes without being able to make channels beyond a few feet in depth. Could such streams have scooped out cauldrons from 500 to 1,000 feet in depth and from half a mile to a mile in breadth ?" (Mackintosh, *ibid.,* pp. 199, 200).

Valleys enclosed at their heads by solid rock walls are only exaggerated cooms, to the erosion of which similar arguments apply. There are numerous deep valleys in the Alps that are closed at one end by steep mountains or perpendicular walls of rock, and are now nearly closed at the other end. Such are the valley of Thones, near Annecy, the valley of Chamouni, and on a larger scale the valley of Geneva. It is probable that the valley of Thones and that of Geneva have once been filled with water and formed lakes. By an earthquake, or by the erosion of water, a fissure has been made, which has drained the greater part of these valleys ; but it is obvious that the valleys could not have been formed by the original lakes or by rivers that flowed into them.

Again : " Some valleys, as Les Eschelles, near Chambéry, are closed at one end by a perpendicular wall of rock ; through this rock a tunnel has been cut for the road ; but it is impossible to conceive that any action of water courses would have formed such a valley. There is only a feeble stream that flows from it. Malham Cove, at the head of the valley of the Aire in Yorkshire, is a perpendicular wall of limestone 200 feet high ; at its feet the river rises, but no conceivable action of the river could have originally formed this valley. Whatever extension we may reasonably grant to the action of rivers, it will not be found sufficient for the excavation of valleys except in particular situations " (*Introduction to Geology,* pp. 583-585).

This concludes what I have to say about the erosion of the earth's surface by running streams and rivers, and I trust I have shown that the normal school of erosionists, who base their teaching on Hutton, Playfair, and their scholars, have exaggerated the potency of this cause to an enormous extent, and that it cannot be made responsible for fashioning some of the more prominent features of a landscape, its valleys, lakes, gorges, ravines, etc. I will now say a few words upon running water, not on the surface, but acting underneath the ground in the form of subterranean streams. Here again it seems to me that a most fallacious induction has been followed. Because the water is now flowing in these underground caverns, therefore it is argued that the water has made them. Just as if because a part of my head happens to be in my silk hat it is a proof that my head made that hideous deformity.

If we examine the caverns which are chiefly found in limestone districts we shall find that in many cases there are no streams running in them, while in others there is no water at all, or the water is resting in eternal quietude in deep pools.

Instead of being eroded they are being filled up by deposits of carbonate of lime and coffined in. This deposit covers their floors and lines their ceilings, and makes pillars and other cave furniture. No doubt some of them do contain water which runs through them, simply because it has found an easy passage.

If we examine the contour of these caverns, look at their ground plans, draw sections of them, see the sharply re-entering angles and exceedingly irregular outlines they make, look at their perpendicular walls cut as if with a knife, and in many cases with benches, projections and great irregularities of surface, we shall perhaps learn a notable lesson. How are we to attribute these to the eroding action of water, either loaded or unloaded. Water when it erodes a passage does so with definite curved outlines, which secure for it a path of least resistance. It does not make staircases or the irregularities dear to a golf-player. Every feature of these cavernous hollows bespeaks for them, it seems to me, another and very different origin than that of aqueous erosion. I have walked for miles up and down the irregular pathways in the caverns of Han, near Rochefort in Belgium, with amazement, and it

seems to me literally impossible to believe that these great, deep pits, these rugged, tumbling surfaces, these angular projections everywhere, can be attributed to water erosion, and the same is true of many other caverns, of which those at Rochefort, and the more famous caves at Adelsberg and Planina near Laibach, are merely the types.   The rivers in such caves are there because they find convenient hollows ready made for them, and so far as I can judge they had as much to do with making the holes as the water we drink has had to do with making the water-pipes in which it flows.

Let us now turn our attention to another form of erosion to which much has been attributed, namely, marine erosion. There is much greater analogy between the sea and a river when considered as dynamical agents than would at first appear.   The chief difference is that over a large part of the ocean the water, except at the surface, is still, and it is only in certain narrow channels, where the tide flows very rapidly, that the nether parts of the great ocean are in motion at all. It is quite obvious that where the water is still there can be no denudation at all going on, and that over all the vast area in question there is a deposition of materials in progress just as we have shown there is in the larger part of the courses of rivers.   The most recent and authoritative soundings show that over the greater part of the ocean bed there is a deposit going on largely due to the accumulation of the minute shells of marine animals.   Otherwise, so far as we can make out, the denuding operations of the ocean are limited to the margins between high and low water mark around our coasts, where the sea, with the assistance of its chief tool, the shingle, eats away the exposed coast and grinds down the materials into sand and pebbles ; and secondly, to those very local cases, where the sea rushes very rapidly through a narrow channel forming a *race*, when, if it has the assistance of stones in its grasp, it may succeed in scooping out a deeper hollow.

There are no doubt great and numerous inequalities under the sea as there are on dry land.   Mr. Mackintosh is quite right when he speaks as follows : " Under the Atlantic there are escarpments, cliffs, tablelands, valleys, basins and troughs, and even in the shallow seas around the British Isles there

are very considerable deviations from the uniform surface. In the English Channel there is a remarkable depression in the shape of a long narrow ditch, or rather a series of long pits, forming parts of a hexagonal-shaped circuitous line of deepest water surrounding the British Isles. In the English Channel it embraces North Deep, South Deep, West Deep and Hurds Dyke, 240 feet deeper than the surrounding ground. Off the coast of Lincolnshire it embraces the silver pits."

All this is very true, but it is an utterly inconsequent step from these facts to the conclusion that the irregularities in question are caused by the sea or could be so caused under any conditions. Still water can neither excavate nor denude, and as a matter of fact, wherever we examine these irregular surfaces as they have been tested by the sounding lead, they have been found, except very locally, to be covered by sand and mud, etc., so that instead of denuding, the sea is continually covering the wounds and the irregularities in its floor with a mantle and cushion of soft materials.

This is the case, and must have always been the case, over ninety-nine hundredths of the sea bottom.

If we turn from that part of the ocean which is still to that which is in motion we shall have to make the same distinction as we made in the case of rivers, namely, to sharply distinguish between sea water which is clean and sea water which is loaded with stones and other hard material.

Where the sea is running at a great pace through narrow channels which have long been scoured out and where it carries no stones, I believe it to be as incapable of eroding solid rocks as a river is.

We can see in a small way how little effect water has under these conditions when it beats against lighthouses or rushes against solid piers of stone, which it has done for many decades without the smallest apparent effect on the surface.

Mackintosh quotes Sir Henry de la Bêche as having long ago shown that the currents of the Bristol Channel, where there is a great "race," are very powerless even to remove sand, and admits that he had not been able to discover direct proofs that the small shallow current which runs between Brean Island and Breanbeck Cove, under the new pier at Weston-super-Mare, is capable of removing stones. He says

Mr. Pooley, F.G.S., of Weston-super-Mare, had seen no indication of the tidal current which is divided by the wedge-shaped promontory of Brean Downs having worn away the rocks (*Scenery of England*, p. 27).

Mr. Shaw says : " The fragile Wealden sandstones to the east of Hastings stretch out to seawards for many hundred feet as level reefs just appearing above low water, and even the soft London clay spreads out on the Suffolk coast as a level plateau exposed at low tide. There can be no stronger evidence of the impotence of the sea to do much in the way of erosion below the tidal range " (*Geol. Mag.*, iii., p. 449).

I may add that the way in which the zoophytes build their coral houses all over the tropical part of the Pacific is another striking piece of evidence in the same behalf.

Mr. Murray, speaking of the Needles of the Isle of Wight, said : " The uncontrolled pressure of the Atlantic, aided by the most rapid currents known on the shores of Britain, are unable to complete the work of oceanic devastation effected in a former age of the world, by destroying these apparently feeble obelisks. The experienced engineers who built the storm-braving lighthouses of Skerryvore and Dubh Artach on isolated breaker-battered rocks in the midst of the Atlantic, rarely rising above the tides, were not deterred by geologists' tales of the power of waves to consume solid rock. The evidence of the barnacles and seaweeds adhering to the surface of those rocks proves how baseless is the fable of wave erosion. Even the terrible surf-wave of the tropics has for ages lashed the foot of the cliffs at Angola without encroaching on them, though it pulverises to atoms the fragments of the hardest rocks and shells which fall within its swirls. . . . Even the Isthmus of Spurn Point at the mouth of the Humber, which is especially relied upon by erosionists to prove the inroads of the sea, though composed only of a heap of loose pebbles and sand, and exposed to two strong currents, may perhaps be little changed for ages to come; such is the efficacy of long equal slopes and a pebbly sand in repelling the rage of the sea " (Phillip's *Yorkshire*, p. 69; *Scepticism in Geology*, pp. 91-93).

No doubt on exposed coasts, where the cliffs are formed of clay, sand and other soft materials, as on the east coast of

England, in Yorkshire, East Anglia, and at Bognor in Sussex, the waves, assisted by local springs which have undermined the ground, have made a considerable invasion of the land in certain places, and whole hamlets are indeed said to have disappeared, but this has been compensated for by the accumulating of corresponding silt and mud-banks elsewhere, as in the Wash, in Romney Marsh, etc., so that it is hard to measure whether there has been loss or gain. The best proof of the small ultimate effect of the sea as a denuder, even when it flows fast, is in the English Channel, where we have the Roman lighthouse at Dover still standing on the headland where it must have stood in Roman times, while the corresponding lighthouse planted by Caligula at Boulogne has only disappeared within the last two centuries. Similarly, the Solent must have been very much what it is now in the days of the Romans, as we may gather from the account Diodorus gives us of the trade route for tin.

Sir A. Geikie speaks on this subject with a sobriety and wisdom which have not always been imitated in these inquiries. " Islanders as we are," he says, " and familiar from infancy with the fury of the breakers which beat along our coast line and strew it with wrecks, we are prone to attribute to the ocean the chief share of work in wearing down the land. Yet if we attentively consider the abrasion due directly to marine action, we are led to perceive that its extent is comparatively small. In what is called marine denudation the part played by the sea is mainly that of removing what has already been loosened and decomposed by atmospheric agents. When these decayed portions are carried away a fresh surface is again laid open to subaerial influences, to be in turn reduced to fragments and borne away seawards. . . . Let us grant to the action of the waves and tides all that is usually included under the term *marine denudation*, and we shall still find that the sum total of waste along the margin of the land must be trifling compared with that which is produced by the meteoric agents upon the interior."

The same writer asks pertinently : " If the sea plays so transcendent a part in the denudation of the land, why should there be such widespread continuity between the seaward and landward slope of the land in shelving coasts ? It must be

granted," he says, " that the erosive action of the sea is almost wholly confined to the littoral waters. Whatever lies below the influence of winds and waves can suffer but little change. The tides and breakers breaking on the land cut away a notch or platform along its margin, and the surface of this platform comes in the end to correspond with the downward limit of breaker action. Now, if the ocean as a denuding agent really deserves the prominent place usually assigned to it, why should there be this common seaward prolongation of the land slope, etc. ? " (*Op. cit.*, pp. 182-186.)

How little effective as an eroder the sea is when not charged with shingle may be seen in the so-called glacial striæ which have been noticed in the fiords of Norway, in America and elsewhere, running down the face of the rocks far below low water, and which must have been there since the last geological period.

As a matter of fact the unloaded waters of the sea are, at the best, mere porters of loose materials and not eroders at all. It is no doubt very different when the waves of the sea are loaded and armed with great masses of shingle and stones. Then they do become powerful and sometimes overwhelming agents of destruction, but we must remember in saying this how very limited the process and its effects are compared with the demands made upon it by the advocates of marine erosion. First, waves carrying shingle only work on sea coasts. Secondly, they only work where shingle exists : the great proportion of coast lines are sandy and not stony, and although sand in great masses and in motion can erode somewhat, its work in this regard is very small. Thirdly, the shingle must have something to work upon, that is to say, it is only where the shores are rocky or where there are cliffs against which the shingle can beat that it can work. This again limits our area very considerably. But this is not all. In a great many cases the cliffs in question have got no beaches in front of them, but the deep water comes right up to them. Here again, therefore, the materials are very slight for doing mechanical work.

" Some writers," says Kinahan, " delight in describing the sea as throwing its breakers loaded with stones and shingle against cliffs, and thus wearing down even the hardest rocks.

The idea ' of nature's artillery ' battering down a cliff may be very poetical, but it involves a misconception as to fact. The whole of the west coast of Ireland is open to the full force of the Atlantic waves, which often rise hundreds of feet, ' blue water ' having on one occasion carried away the water tanks at the upper lighthouse on the great Skellig, which, according to the ordnance map, is 380 feet above mean tide level ; while spray has been driven clean over the island of Valencia so as to wet the windows in Knightstown, the cliff at the west end of the island being from 500 to 700 feet high ; yet in no place on the west Irish coast did we remark this ' battering-ram ' process going on. On the contrary, in those places most exposed to the waves, the seaweeds usually grow luxuriantly, clothing these rocks with a mantle. Abrasion by the sea will take place in coves, guts and the like, but this is principally due to the ' back wash '. Stones, when carried in by the waves, fall on a cushion of water, and the force of the fall is broken, but as the waves retreat they roll in the back wash, over and against one another and the rocks. Consequently it is only in coves, or such like confined places, they are capable of acting as abraders " (Kinahan, *Valleys, Fissures,* etc., pp. 42, 43).

When we have thus limited our problem it will be seen how much smaller it becomes than might at first appeared. The limitation we have made, however, is chiefly one of area. We must also limit the capacity of the tide in regard to the kind of work it can do.

It would appear that even the action of the sea, certainly at times most powerful and important, has a similar tendency to impose a limit on its own ravages. It has obviously in many instances formed an effectual barrier against itself by throwing up shingly banks and marsh lands in face of cliffs against which it once beat ; and after the destruction has been carried to a certain point, it seems necessary from the mode of action (excepting where very powerful currents interfere) that the very materials resulting from the ruin should check its further increase ; even where these currents exist they also have a tendency to throw up barriers of shingle in their eddy.

Turning away from the general question to the particular kind of marine denudation more immediately germane to our discussion, I find that Prof. Hull, Mr. Mackintosh and other

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champions of marine erosion have failed to give us examples
of what the sea is doing at all resembling the work of ex-
cavating valleys, gorges, rifts, etc., extending for miles with
straight-up sides and clean-cut edges. There is nothing like
it in the whole world going on, so far as I know, and the
whole theory, however ingenious, is not based on induction.

The waves of the sea, when armed with stones, can cut
back cliffs sometimes for a certain distance, especially when
the lower layers of the strata are soft and yielding, and can
hollow sea caves and coves just like a river can hollow out
rounded hollows.

The process is well described by Kinahan. He says : " The
rock, or rather the débris of the rocks (fault rock), included
between the walls of a fissure is in many cases softer and
more easily denuded than the associated rocks. The waves
will work with vastly greater effect on such fissures than on
the adjoining rocks, and thereby excavate long narrow guts
that may be of great length, although only a few yards wide.
Many straits have evidently been so formed, the sea working
along and excavating out a dyke of fault rock across a pro-
montory, thereby eventually forming an open fissure, and
dividing one portion of the land from another. Some of these
guts are open to the sky, but others may occur as long narrow
caves ; the latter instances proving that the sea is capable of
working along the fault rock much faster than meteoric
abrasion " (Kinahan, *op. cit.*, pp. 57, 58).

Where the coast consists of granite or other jointed rocks the
sea can beat against these and remove blocks and wear away
the softer parts which sometimes separate the hard zones, and
in this way can create in certain places a coast with a ragged
and fantastic outline, showing archways and detached pillars,
etc. All this is granted ; but all this has as much to do with
the problem of explaining the river valleys and the gorges as
a mason's work has with the formation of the Matterhorn.

Nothing can be more unlike a marine excavation than such
gorges as that at Cheddar, and it is curious Mr. Mackintosh
did not apply his own simile to his own argument, namely, that
a Somersetshire farmer can easily distinguish a cheese which
has been nibbled by a mouse from one cut with a cheese scoop.

The fiords of Norway, Greenland, etc., many of which have

ramifying head fiords and inlets, are perhaps even more unlike the work of the sea. The fact that they are hollowed out far below the level of the sea bottom, and rise more or less abruptly at their foot, seems conclusive in this regard, since the tide could not excavate narrow deep hollows of this kind. Besides, the sea within the fiords is protected from storm and tempest, and is more or less quiescent, nor is it there armed with shingle and stones.

Again, as Mr. Shaw urges, motion cannot take place up a *cul-de-sac;* "a cushion of still water" would fill the recess, deflecting the current at its mouth, and thus neutralising its excavating power; and again, the action of waves within an inlet, be it ever so straight and exposed, must always be less than against a headland. A headland is more or less assailable from three quarters, while an inlet can only receive the first force of the direct wave (*Geol. Mag.*, iii., pp. 448, 449).

How, again, are the valleys with their sloping beds, or the cooms before which the ground slopes away, to be attributed to the effects of marine erosion ? When the sea erodes, it makes a flat surface ; furthermore, all the persistent currents of the ocean are on a scale altogether disproportionate to the details of coast outline, and for the most part take grand sweeps parallel with the coasts (*Geol. Mag.*, iii., p. 449).

Let us, lastly, say a few words about one critical factor in the various current theories about the denudation of the Weald, which is dependent on marine erosion.

Every person who has written on the Weald and its erosion recently has treated the Weald area as sharply cut off on the east by the sea, and the problem as one defined in a measure by this fact. If we wish to realise it as it really is, however, we ought to examine the other side of the Channel, when we shall see that the lines of the Chalk Downs converge again and meet, and that the lines of the upper greensand do the same in the Boulonnais; and we can. hardly doubt when we examine the lines in this continental area that when the Wealden valley was quite intact, instead of forming a great triangular area with its base running from Beachy Head to Folkestone, it really formed a much larger and lenticular-shaped district, terminating in the west near Petersfield and in the east at Fauquembergues in the Pas-de-Calais.

We shall further note that the erosion of the continental part of the area was more complete than that of the English, for it seems to have cleaned out the upper Jurassic beds that form the true Wealden elevation and laid bare the Kimmeridge and Oxford clays below.

Those who invoke marine erosion as having had a principal part in starting the denudation of the great valley must cease to treat the Wealden area as a bay-like prolongation of the English Channel. When intact its shape shows that if eroded by the sea it must have been by a sea of its own.

Mr. Topley, following Ramsay, says the sea, wherever we now see it at work, exerts a levelling power, planing off to a more or less uniform slope the various beds which come within range of its breakers. This forms what Prof. Ramsay has called a plane of marine denudation, but, as Mackintosh argues, the bottom of the sea is as uneven as the dry land. We mistake the sea bottom for the level bit of strand between high water mark and low. If we go out into deep water we shall at once find inequalities like those occurring in any terrestrial landscape, and I know of no warrant whatever for the position so often assumed that the eating back of coasts by slow marine erosion has ever done more than erode a comparatively small area round the outside of the present land margin.

The Wealden area is at present a vast amphitheatre from twenty to forty miles from north to south, and nearly eighty miles from east to west. The ordinary postulate is that the whole area once formed a dome-shaped elevation covered by chalk, upper greensand, etc., and that the sea originally broke down the roof of chalk and thereby created a plane of marine erosion.

Topley and Foster begin their argument by postulating the removal by marine denudation of a large portion of the tertiary and cretaceous beds from the Weald area. How an area of this size and shape could have had its chalk covering originally scooped out by the sea I know not. We have seen the kind of erosion which the sea performs. Again, how, if the sea did it, did it come about that it left no traces whatever of its having been there? Where the débris of this denudation are to be found I know still less. The toughest

morsels for the denuding tooth of time to work upon are chalk flints. These are entirely absent from the Wealden district except close to the Downs. Where are they gone to if the chalk has been broken away and swept out? The flints ought to have remained somewhere—some pockets, some detached groups, even if the great mass were swept out to sea. But I know of none, nor do I know of any traces in the English Channel itself in the shape of great beds and heaps of gravel.

This is the position long ago maintained by Murchison. He says in his *Siluria,* in answer to Lyell, who began by being an advocate of marine erosion and afterwards changed his views, that " there is no proof whatever that the waves of the sea ever beat against or wore away the escarpments of either the North or South Downs ; for if such had been the case we should surely, somewhere in the great circuit of the chalk cliffs which subtend the Wealden area for a distance of 160 miles, be able to detect some evidence of such shore action, whether it took place during the latest tertiary or at any subsequent period. Gravel beds formed by the breakers, whatever may be the date of their formation, are always clearly distinguishable by their rounded condition from all other kinds of drift translated by water. . . . In these cases the pebbles are just like the shingle now rounded on the shore. But not a trace of such shore action is to be detected at any level, at the foot or along the sides of the escarpments of the North and South Downs. In place of this we only find these local heaps of angular and broken flints. . . . Further, if we look to the interior of this vast valley, we find the dome-shaped centre, consisting of the Weald clay and Hastings sand, just as entirely swept of any superficial frag-ment as the smaller valley of Woolhope, except in those limited tracts of narrow dimensions where rivers of date long posterior to the great denudations have acted. . . . By what ordinary currents of the sea, I ask, could such a sweeping out be made of such an area of nearly 200 square miles. Except where some subsequent river, like the Medway, has flowed and left some shingle and gravel, every portion of the soil is but the decomposition of the denuded rock beneath " (*op. cit.,* pp. 493, 494).

To continue, however. When the sea had done its work a comparatively plane surface, it is urged, "was formed which gradually appeared above the water; probably the centre of the Wealden area arose out first, forming an island, and then as the land rose a spread of country was formed sloping down to the north and south from an east and west ridge " (*ibid.*, p. 473). It was upon this plane of marine denudation that rain and rivers are supposed to have acted.

I do not propose to continue the analysis of this part of the problem further, as I have already dealt with the notion that the Weald was denuded by fluvial or pluvial action and shown it to be untenable, but will conclude with a few words of criticism on the general notion maintained pertinaciously by Mackintosh that the great lines of inland cliffs and scarps of the Weald were eroded by the sea.

In regard to these escarpments, Messrs. Topley and Foster and Mr. Whitaker's case against marine erosion is effectively handled by them. Thus they enumerate some of the reasons against the submarine origin of the features in question which have been urged by themselves and others, and which it is well to quote again.

1. Escarpments always run along the strike, changing their direction as the strike changes, while sea cliffs rarely do so, and then only for a short way, but cut through rocks without regard to it.

2. The bottom of the escarpment, either of the chalk or greensand, does not keep to one level all round the Weald as it would if coincident with a line of marine erosion, but rises slowly inland or towards the watershed, and sometimes the base at one plane is higher than the top at another.

3. Sea cliffs are generally tolerably straight, or run in curves of large radius, while escarpments run in very zig-zag or meandering lines.

4. If the escarpments were formed by the sea they ought to have beaches at their feet, which they have not, and, as Murchison long ago pointed out, there are neither pebbles nor sea shells to mark anything of the kind.

5. Sometimes two escarpments facing the same way, and of different rocks, run roughly parallel together for miles. This, as Ramsay argued, would necessitate the postulate of a

narrow strip of sea between two parallel ridges of land, each of a different formation. Anything like this is unknown elsewhere. Nor would the sea in such a case have force enough to erode, and in such a case the chalk escarpment ought to be the smaller, since it is inside and more sheltered, while it is really the bigger.

To these reasons Mr. Whitaker adds three more :—

6. The tops of escarpments are for the most part even and nearly flat, while those of cliffs are uneven. .

7. Escarpments generally form the highest ground of a country, while sea cliffs often have higher ground behind them.

8. The escarpments of successive formations run in more or less parallel lines for long distances with plains, vales or valleys between, while sea cliffs where no such parallel arrangement is known.

While these geologists, however, deprecate attributing the cutting back of the Weald escarpment to the action of the sea, they follow Jukes in assigning to marine denudation the erosion and removal of the old chalk covering of the Weald area, which is an even more fantastic notion, and which is a necessary preliminary to their subaerial denudation of the nether beds. To this latter notion I have already referred sufficiently.

This completes what I have to say about marine erosion. Where the tide flows fast the sea can and does wear away the soft cliffs and removes the impediments to its passage. Where it is armed with hard rough shingle it will scoop out caves and arches and the other fantastic architecture of the cliffs when formed of hard intractable rocks; where it runs in a rapid " race " it can no doubt scour out a channel ; but these are local and comparatively small results compared with those generally attributed to it, and have done very little to shape the surface of the earth.

CHAPTER VIII.

ICE AS AN ERODER AND EXCAVATOR.

" The physicists of the future will, I feel confident, point out the impossibility of glaciers acting capriciously under identical conditions, and will insist on the incongruity of arguing that an ice stream which did not widen the gorge of St. Maurice, on emerging from the mountains excavated the lake of Geneva, or that a natural agent, which was baffled by the crags of Sion and deflected by the ridge of the Forclaz, could have cut the valley of the Rhône."—Freshfield, *Geographical Proceedings*, 1888, p. 789.

THE two previous chapters have dealt with a part of my subject which was not touched upon in my earlier work on *The Glacial Nightmare.* I now have to treat of an issue which occupied me at great length there, and what I have to say must be considered as ancillary and supplementary to what I then wrote.

In the previous chapters I have been dealing with the denuding and eroding effects of subaerial agents other than frost. I must now turn to that potent and very efficient modeller of the earth's surface. Wherever the ground is bare and unprotected by herbage or moss or snow, and the temperature is sufficiently low, the biting tooth of frost is actively at work, and it must have been so since the world was first made. Where frost has alternated with intervals of considerable heat the process of decay has been more rapid. The mere shrinking and swelling influences of alternate cold and heat have disintegrated the bare rocks and the more friable and loose beds. Wherever, again, joints or fissures were permeable to damp, damp has invaded them, and when this damp has frozen, the natural effect has followed. Since frozen water bulks larger than liquid water, the freezing of the damp in these interstices has riven the rocks asunder. Sometimes they have not been disturbed in their beds, but the joints have

been widened so as to convert them into great masses of natural ashlar. Where, however, the frost has been at work on exposed crags or scarps or other masses of rock with steep sides, the shattering of the strata has led to the creation of great masses of ruin at the feet of the exposed cliffs in the form of sloping beds of splinters and sharp-angled blocks and sand known as screes or talus. We cannot travel long in an upper Alpine valley without hearing the sound of some stony avalanche or of the rolling or sliding down of rubbish, as the high surfaces disintegrated and loosened by the frost pour down their tribute to the valley below. This has been going on for untold ages, and accounts, no doubt, for the scarified and rough and mangled look of so many mountains whose surfaces have been thus shaped by the rough tools of alternate cold and heat. In certain favoured localities, where subaerial denudation has been uninterrupted for a long time, the screes are sometimes very great. "Immense accumulations of frost-made talus," says Scott, "are to be found in such places as the foot of the palisades of the Hudson, the abrupt southern slope of the Delaware water gap, and wherever cliffs or peaks of naked rock are exposed to severe cold. Many mountain passes are so bombarded by falling stones as to be extremely dangerous. In the Sierra Nevada of California talus slopes as much as 4,000 feet high are reported, all the work of frost. At Sherman, where the Union Pacific railroad crosses 'the continental divide,' the ground is covered for miles with small, angular fragments of granite broken up by the frost " (Scott, *Introduction to Geology*, pp. 82, 83).

A great deal is made in popular geological text-books of phenomena like this, but they may be and have been greatly exaggerated.

The snow which covers so much of the land at high levels is a great protector, and does a great deal to prevent the action of frost in mountain ranges, for it completely shelters the ground on which it lies from its action. Mountains also are made of different materials. In many cases they are not fissured, and frost cannot penetrate their sides, and we consequently find that their rate of decay is not at all uniform, but the reverse. Again, the amount of this decay may be actually measured in many cases by a more accurate test

than a few rhetorical adjectives. The screes which rest against so many cliffs and hillsides, and which have not been carried away or weathered, of which myriads exist, are a proof that the amount of decay that has gone on since the mountains and valleys received their present contour has not been interminable and without limit, but limited by very narrow considerations. A great many of the mountain screes are covered with grass, just as the talus is on parts of the coast near Dover and elsewhere, showing that the surface decay has largely stopped, while, if we put them all together and try and equate the sum with the demand made by the extreme champions of erosion, who would have us believe that the mountains were carved out of their matrix by subaerial causes, especially by frost, we shall have a fair measure of the demands made on our credulity. The measure here referred to is quite as efficient in the mountains of Scotland, Cumberland and Wales as it is in the Alps and the Himalayas, and is a very good tonic to the overheated imagination which can manufacture great dreams out of small facts.

Let us proceed, however. All the débris which falls from exposed mountains does not remain in the screes. That is quite true. A portion of it is carried away by the streams, as we have already noted, and another portion is carried away by the glaciers, and it is to glaciers and their handiwork I will now turn.

Water freezes in several ways. When frost attacks it in a pool or a lake it freezes in a more or less continuous crystalline form without any internal gaps or fissures or granulations. Such ice, so far as we know, is immovable, save the mere stretching and shrinking which it undergoes under the influence of warmth or extreme cold. We have all often heard when skating on very cold nights the sharp noises caused by the cracks the ice has made in shrinking, and have, on the other hand, seen the shingle, etc., it has moved up on the lake sides when it has stretched itself.

What takes place in a small way on our homely ponds occurs on a much larger scale in big northern rivers, where the surface is frozen all over, as the St. Lawrence and the Siberian rivers, especially in their broad outlets, and in the case of frozen seas like the Baltic, the Arctic Sea, etc.,

and the works of the early geographers are full of notices, by Captain Bayfield and others, of the removal of large masses of shingle and enormous boulders by the operations of shore ice, which has sometimes pushed them up inland, and at others moved them along the coast or in the direction of the drift of the ice. We shall have more to say to this kind of work presently. Here it will suffice to remark that *the erosive* effects of such ice as we are talking about is very small and very local, and is quite a negligeable quantity when dealing with the great problem before us.

Let us now turn to ice in another form. The frozen moisture in the air falls to the ground in two forms, namely, as hail and snow. Hail, consisting of spherules of ice arranged in concentric layers and sometimes in larger crystals, is a form of frozen rain. Such frozen rain is sometimes very destructive—the stripping of fruit trees of their fruit and leaves, the destruction of crops, the killing of animals, the beating down of fragile structures, and the battering and disintegration of more or less loose deposits are familiar results of a heavy hailstorm ; and in the tropics, where hailstorms are more frequent and the stones themselves are often of larger size, the destruction in the ways mentioned is a very considerable one. But this, again, is relatively a trivial matter. It is as snow, or rather as modified snow, that the most important dynamical work is done by frozen water.

Snow is formed of tiny ice crystals. How it is formed I prefer to describe in the graphic and beautiful phraseology of that word painter, Tyndall. " Snow perfectly formed," he says, " is not an irregular aggregation of ice particles ; in a calm atmosphere the atoms arrange themselves so as to form the most exquisite figures. You have seen those six-petalled flowers which show themselves within a block of ice when a beam of heat is sent through it. The snow crystals, formed in a calm atmosphere, are built upon the same type ; the molecules arrange themselves to form hexagonal stars. From a central nucleus shoot six spiculae, every two of which are separated by an angle of 60°. From these central ribs smaller spiculae shoot right and left, with unerring fidelity to the angle of 60°, and from these, again, other smaller ones diverge at the same angle. The six-leaved blossoms assume

the most wonderful variety of form, their tracery is of the finest frozen gauze, and round about their corners other rosettes of smaller dimensions often cling. Beauty is super-imposed upon beauty, as if nature, once committed to her task, took delight in showing even within the narrowest limits the wealth of her resources. These frozen blossoms constitute our mountain snows; they load the Alpine heights, where their frail architecture is soon destroyed by the weather. Every winter they fall and every summer they disappear; but this rhythmic action does not perfectly compensate itself. Below a certain line warmth is predominant, and the quantity which falls every winter is entirely swept away; above this line cold is predominant, and the quantity which falls is in excess of the quantity melted, and an annual residue remains. In winter the snows reach to the plains, in summer they retreat to the *snow line*, to that particular line where the snow-fall of every year is exactly balanced by the consumption, and above which is the region of eternal snows " (*Heat as a Mode of Motion*, pp. 176, 177).

The snow line is a limit of great moment in all discussions on meteorology. Its height above the sea level varies according to the latitude—at the poles it is near the sea level, while in the tropics it is many thousands of feet above it; at the equator it is about 16,000 feet high. This is not the only important meteorological line on mountains; another is that marking where the temperature never sinks below the freezing point of water, and where there is no rain and no thaw. Above this line the snow remains dry snow, the only melting is a mere veneer on the surface due to occasional fierce rays of direct sun-heat. So long as it remains dry, snow remains a mere dust heap of incoherent disintegrated snow flowers, blown hither and thither by the wind and incapable either of erosion or any other dynamical work; and it may under certain conditions remain in the condition of mere loose snow for a long time, as it does north of the Himalayas in Tibet, on the western slopes of the Dovre Fjeld and the eastern ones of the Andes, where large portions of it never coalesce, but always remain pulverulent. There is a constant ablation going on on the surface of this dry snow by a process of sublimation, and the dry air becomes somewhat damp in

consequence; but there is otherwise no change. In such places glaciers are non-existent, and are not in fact possible.

Over the greater part of the world's surface, however, where snow falls it does not remain dry. What takes place may best be described in Ruskin's graphic phrases. I will slightly condense what he says. He tells us of a hollow in rocks at the summit of a mountain above the line of perpetual snow. The snow once fallen in this hollow cannot get out again; but a little of it is taken away every year, partly by the heat of the ground below, partly by surface sunshine and evaporation, partly by filtration of water from above, while it is also saturated with water in thaw-time up to the level of water-shed. Consequently, it must subside every year in the middle, while a similar quantity is added every year at the top. Hence the entire mass will be composed at any given time of a series of beds, more remaining of each year's snow in proportion to its youth, and very little indeed of the lowest and oldest bed. Meanwhile every interstice and fissure in the snow during summer is filled either with warm air or warm water in circulation through it, and every separate surface of crystal is undergoing its own degree of diminution.

The effect of heaping up snow is, of course, to cause considerable pressure on its nether parts, and, as we have seen, we have to deal not with freezing but with thawing snow, for, as Forbes showed, the snow is continually thawing in the Alps even in winter, not superficially nor in all places, but internally and in a great many places, and all the year round you must reason on the aqueous deposits on the Alps as practically in a state of squash; not freezing ice or snow, nor dry ice or snow, but in places saturated with, everywhere affected by, moisture, " subject to the influence of a subsiding languor of its fainting mass " (*Deucalion*, i., *passim*). The result of this is that the snow is rapidly changed, first into semi-ice, called *névé* by the French and *Firn* by the Germans, and then into true glacier ice, just as the damp snow in the schoolboy's hands is squeezed into a semi-transparent snow-ball. The whole rationale of it was worked out by Tyndall in his investigations on regelation.

The chief difference between blue ice and opaque névé is that the latter encloses a great quantity of air, which gets

entangled among its particles. It is in fact of a more cancellous structure and much lighter in weight. Its conversion into true ice depends apparently upon two factors—plenty of moisture, and a sufficient pressure to squeeze out the air particles and cause the frozen particles to get into closer contact. It is probable that in certain countries, like Greenland and the Antarctic regions, where the temperature is never high, the perfect conversion of névé into blue ice only occurs locally, and not generally—an important fact to which I shall revert presently. Let us move on, however.

The result of what I have been describing is that when the snow is thus converted into ice, the ice is very different indeed to pond ice or ice made in a laboratory. It is not a continuous crystal, but, as was shown by Hugi and Forel, and as I explained at length in my previous volumes on *The Glacial Nightmare* (p. 528, etc.), it is granular, and has a discrete instead of a continuous structure. The importance of this we shall see presently. The effect of the whole process is, roughly, that while the tail of a glacier is made of hard blue ice, its middle consists of névé and its crown of snow, the very top of the crown consisting of the last snow-fall and being composed of dry snow.

These rivers of ice, névé and snow filling the bottoms of the Alpine valleys are, as has been known for a long time (*Glacial Nightmare*, pp. 522, 523), constantly moving slowly downwards. Why and how do they move? In judging of the views and the conclusions of the earlier champions of the Glacial nightmare on this question, we must always remember that they looked upon glacier motion quite in a different way to that in which we are accustomed to look at it. They looked upon a glacier as a rigid solid body, and when it was proved beyond all doubt that a glacier when lying in a sloping valley can and does move, its motion was treated as that of a solid body, and several different theories were published about it by the Swiss and other geologists. One of these was that ice moves down its channel as a solid body *en masse*. I have described at some length in my former work on *The Glacial Nightmare* (ii., pp. 545-554) the origin and various modifications of this theory, and the objections which have been made to it. These I shall not now repeat.

When I wrote my former work I was under the impression that while the motion of a glacier is, as Forbes has proved, the movement of a viscous body, there is a certain small movement of it *en masse* due to sliding on its bed. I am not now quite so sure of this, and am disposed to think that whatever motion there may be in the lowest layers of a glacier as a whole is largely a motion induced by the drag of its upper layers and a purely viscous one.

If there be any such motion *en masse* it cannot be great, nor can it exceed a certain amount without the force inducing it becoming dissipated. This seems plain, as I showed before from some simple considerations. Every solid known to us will crush and disintegrate under a sufficient pressure, and it does not matter whether this pressure is applied perpendicularly downwards, or laterally. It follows, therefore, that if a solid be so heavy and so big that it requires more than a certain force to move it, it will crush rather than move, that is to say, the whole thrust will be dissipated by the object being reduced to pulp, or even liquid, which will flow away rather than move *en masse.*

This argument applies to all solids, and notably to what is almost a solid, *i.e.,* to ice. The crushing point of ice has been roughly ascertained. It enables us positively to say that a mass of ice which is longer than (according to Oldham in his paper on the modulus of ice) about seven miles cannot be moved *en bloc* along a flat surface without crushing. If the ice has to move up-hill, and therefore to overcome gravity, the difficulty of moving it *en masse* will, *a fortiori,* be increased, and the length of the column of ice capable of being moved will be proportionately lessened. If it is on a slope and gravity gives its assistance, this motion will be reversed, and the greater the slope the greater the distance to which the mass can be moved. This is of course treating the problem apart from friction. There is also evidence that when glaciers reach level ground their motion, however caused, rapidly ceases.

While some of the Swiss geologists adopted the sliding theory, others, as I have shown (*Glacial Nightmare,* p. 524, etc.), adopted the dilatation or infiltration theory. This has been revived again in our time by Dr. Drygalski and also by Viollet-le-Duc in his work, *Le Massif de Mont Blanc,* who has in effect

combined the two theories just named, and urged that on a sufficient slope a glacier moves *en masse*, while when it gets on more level ground it moves by dilatation. Dr. Drygalski apparently attributes the movement of the ice largely to the oscillation of temperature, and therefore of bulk, in the interior of the glacier, caused by water reaching the interior and thus raising the temperature of the lower layers, which he maintains are chiefly concerned in the motion of the ice. Against this it is conclusive that the facts seem clearly to establish that it is the surface layers in glaciers which move the fastest, and not the nether ones.

I have devoted several paragraphs to the refutation of this notion of dilatation in my former work (*ibid.*, pp. 525-528), and will here merely content myself with quoting another passage from Ruskin in regard to it :—

" Those who hold this theory suppose that when a shower of rain falls on a glacier the said rain freezes inside of it. Another form of the dilatation theory is that a glacier expands by freezing its own meltings. The result," as he says sarcastically, " is supposed to be that the glacier, being thereby made bigger, stretches itself uniformly in one direction, and never in any other, also that, although it can only be thus expanded in cold and wet weather, such expansion is the reason that it always goes fastest in hot and dry weather " (*Deucalion*, i., pp. 185-187).

A third theory, which originated, I believe, in England, and was chiefly pressed by Mosley, treated glaciers as bodies subject to alternate expansion and contraction by heat and cold like other solids, and accounted for their movement accordingly. The premise that ice, like other solid and semi-solid substances, is subject to these pulsations is not an un-reasonable one, although the conduct of water in the solid state is different to that of most other substances, since it does not contract but expands on becoming solid, and only begins to contract again at a certain temperature.

The theory of the movement of glaciers being due to alternate expansion and contraction will not stand criticism, however. I have quoted several adequate replies to it in my *Glacial Nightmare* (pp. 523-535), and will here only add some additional remarks upon it by Mr. I. C. Russell, an American

geologist. After remarking that, if it were true, the ice ought to swell up in the direction of least resistance instead of progressing downwards, he refers to the slow conductivity of both ice and snow and the manner in which glaciers are invariably blanketed with snow throughout a large part of the year. For example, in névé regions, the loose granular snow is frequently hundreds of feet deep, and is not only an exceedingly poor conductor of heat, but, on account of its open texture, would undergo but slight changes in mass, on account of changes in temperature, since slight movements of the granules would be taken up by the adjacent interspace. It does not require accurate observations to show that in such regions changes of temperature are too brief to be felt at any considerable depth, and even under the most extreme conditions could not cause sufficient change to account for the flow known to occur in névés (*Glaciers of North America*, pp. 177, 178).

I have nothing to add to this. The hypothesis is in fact dead. The various theories above described all ceased to have a *locus standi* when Forbes, by his insight and experiments, clearly showed, in the first place, that a glacier is not a rigid body, and, secondly, that it progresses by the differential movement of its particles. He boldly declared, as the result of his experiments, that ice is simply a viscous substance and moves as such (*vide Glacial Nightmare*, pp. 554-574). The publication of these experiments by Forbes was received by Agassiz, who had failed after many years of observation to find the key to the problem, with chagrin and ill humour, which was again reflected in the writings of his friend Desor, and more ignobly in England by Tyndall and Huxley, whose spiteful conduct towards Forbes, who was denied the Royal Society's Copley medal by them, was greatly resented by his friends, and notably by Tait and by Ruskin, the latter of whom has enshrined in his imperishable English a statement of the proceeding, which is not only a fine piece of moral writing, but also a keen and acute piece of scientific reasoning. The story is a sordid one. One of the results of the quarrel was the publication by Tyndall of a rival theory to that of Forbes, *i.e.*, that of fracture and regelation (*ibid.*, p. 576, etc.). I have already criticised Tyndall's theory at length (*ibid.*, pp. 578-

581), and will only add the views of one or two other critics who have taken fresh objections to it.

Mr. I. C. Russell says of it that " the force which causes the motion of the ice is assumed to be its own weight; but in all the experiments which have been made upon regelation a force greater than the weight of the ice experimented upon has been applied. . . . Little has been said in the discussion as to the manner in which glaciers might be crushed so as to make regelation possible. From the experiments of Moseley, a column of ice would have to be 700 feet high before it began to crush. If this be so, glacier ice cannot be crushed under its own weight unless at least 700 feet thick, and then the fracturing would be confined to the bottom layer. We should therefore, under the hypothesis of regelation, expect the greatest freedom of movement to occur in the basal portion of a glacier, yet, as is well known, the maximum movement is at the surface. How, then, can the principle of regelation be applied in explaining the surface flow, especially of a glacier with a low surface gradient? Again, regelation takes place at a temperature of about 32° F., and cannot occur much below that temperature unless the ice is under pressure, and the rate at which the melting point of ice is lowered by pressure is so small that practically it may be ignored in this discussion. Besides, the rate of surface flow of a glacier is greater than the rate below the surface, even in winter, when the temperature of the ice is frequently far below the point where regelation is possible. It seems, therefore, that the regelation hypothesis fails to meet several important features of the problem of glacier motion" (*Glaciers of North America*, pp. 173, 174).

Ruskin's phrases are more sarcastic. " The scientific persons," he says, " who hold that theory suppose that a glacier advances by breaking itself spontaneously into small pieces, and then spontaneously sticking the pieces together again; that it becomes continually larger by a repetition of this operation, and that the enlargement can only take place downwards—that is," he adds, " if your children put a large piece of barley sugar on the staircase landing, it will walk downstairs by alternately cracking and mending itself " (*Deucalion*, p. 187).

The fact is that the measurements and the precision of Forbes's experiments, which showed that the centre of a glacier

moves faster than its sides and its top moves faster than its base, thus explaining the glacier structure curves, completely excluded any theory of the movement of glaciers like those already mentioned. There only remained one real difficulty in the way of the full acceptance of Forbes's positive conclusion that ice is a viscous body and moves as a viscous body, and that was the alleged results of laboratory experiments upon ice. These experiments, which culminated in those made by Prof. Moseley of Cambridge (*Glacial Nightmare*, pp. 574, 575), seemed to conclusively prove that, whatever might be the true explanation of the phenomena reported by Forbes, ice will not shear when force is applied to it, and cannot therefore be viscous.

In so far as these experiments were tried upon pond ice or ice forming a more or less continuous and homogeneous crystalline mass, the conclusion of Moseley seems incontrovertible. McConnell and Kidd, when they tried to cause a deformation of a crystal of homogeneous ice, found that it would deform only in one direction, namely, when a bar of ice was subjected to a sufficient strain with the optic axis of the crystal perpendicular to two of the side faces. These experiments have been repeated and confirmed by Dr. O. Mugge, published in his paper entitled " Ueber die Plasticitat der Eiskrystalle," *Jahrbuch für Mineralogie*, 1895.

The latter are thus condensed by Chamberlin : " Prisms were cut from carefully formed ice in various directions to the principal crystallographic axis, *i.e.*, the optic axis of the crystal, particularly in directions parallel and transverse to it. These were tested by placing their ends on supports and weighing them in the centre. In testing the transverse prisms the optic axis was first placed in a vertical position. The prisms sagged, and their ends were drawn inwards. Optical examinations showed that the optic axis remained normal to the bent surface. Subsequent observations on surfaces fractured for the purpose showed striation and other indications that plates of the crystal parallel to the base of the crystal had sheared upon one another. When similar prisms were placed so that these gliding planes stood on edge, no appreciable results followed, even though greater weights and longer times were employed. . . . The

investigation seems to warrant the important conclusion that crystals yield to deforming forces by the sliding or shearing of the crystalline layers at right angles to the principal axis " (Chamberlin, *Journ. of Geol.*, iii., p. 965).

These experiments fully sustain the conclusions which had been arrived at by Moseley, which seemed, as I have said, to be fatal to the viscous theory of ice movement. Under these circumstances other theories were invented to explain that motion. The most famous of these was the molecular theory of ice movement propounded by Croll (see *Glacial Nightmare*, pp. 538-542).

I have condensed several of the criticisms made upon this theory in the work just named (*ibid.*, pp. 542-545), but I overlooked an examination of it by my friend Mr. Teall, now the distinguished director of the Geological Survey, which was published as long ago as 1880, and which is excellent. Mr. Teall calls Croll's molecular theory " an absurd speculation," and one involving " a pernicious form of reasoning". He says Croll uses the terms *conduction, radiation* and *molecule* in senses in which they are not used by physicists. " Croll's theory," says Teall, " depends on the supposition that heat energy is being continually transmitted through a glacier, from particle to particle, at a time when the temperature of the whole corresponds to the melting point," and he asks how this can be justified by any fact of observation or deduced from known physical principles. Croll, on page 516, argues as if the radiation of heat through ice is a transmission of the energy termed heat from molecule to molecule, while it is not the molecules of ice which transmit the heat but the ether. Radiant heat may be absorbed by ice particles, but it then ceases to be radiation. Croll argued that since a bar of ice at the melting point is of *the same temperature throughout*, if there be any transmission of energy in such a case it must be accompanied by something like alternate thawing and freezing of small particles of ice. In reply, Teall quotes Clerk Maxwell's definition of what conduction of heat implies, namely, its transmission through a body, depending *on in-equality of temperature in adjacent parts of the body*, by which he does not mean molecules, but ponderable parts. It is thus clear that whatever Croll meant by transmission of heat as

energy, it was something different to what physicists call conduction, and it is very difficult to understand how heat can be transferred by any other process.

Another of Croll's postulates is that as ice becomes liquefied its *molecules* shrink by one-tenth in size, and as the molecules of ice become water they can descend in space in consequence, since they take up less room. Molecules, as was pointed out in my previous work, never alter in size; any change that takes place in them, when matter changes from the solid to the liquid form, is by way of rearrangement and not alteration in size.

On page 523 of *Climate and Time*, after quoting a passage from Tyndall in which the latter gives a theoretical explanation of the mode in which water expands when passing from the solid to the liquid form, Croll continues: " It will be obvious, then, that when a crystalline molecule melts it will not merely descend, but capillary attraction will cause it to flow into the interstices between the adjoining molecules ". On this Teall makes the caustic commentary : "Any person who could write in this strain, after quoting the above lucid paragraph from Prof. Tyndall, *must be confusing molar with molecular phenomena* in the most astounding manner. A molecule of a liquid is positively regarded as a sort of inorganic amœba, capable of insinuating itself into molecular interstices by capillary attraction." And he adds the comment that Croll does not seem to realise that physicists look upon a molecule of steam, of water and of ice as precisely the same thing.

Teall then goes on to test Croll's theory in another way. Suppose we use the words particles of ice instead of the technical term molecules, and that they can move as Croll suggests, he asks how it would explain such a fact as the transportation of a large block of rock down a glacial valley. According to the theory, the ice particle only moves down when it is melted. We cannot suppose that all the ice particles on which the ice rests would melt at the same time, or the block would sink in. Suppose half of them were so melted, then, in order that the block may move down the valley, the moving liquid particles must actually push the block of rock over the fixed solid ones. Lastly, Teall points out that the theory in question does not explain the differential movement of glaciers.

After apologising for taking up so much space in demolishing such a crude speculation, he says his only excuse is that the theory was accepted by many geologists who are not physicists (by Mr. Geikie, for instance, in his most valuable book on *The Great Ice Age*). It was therefore necessary for some one to speak out plainly and tell them that, as propounded by Dr. Croll in *Climate and Time*, it is not worth a moment's consideration.

I have quoted this reply at some length because it is an admirable piece of scientific criticism which is not widely known, since it was privately printed, and because Croll's transcendental speculations, from the obscurity of the subject with which they deal and the reckless ingenuity of their author, have taken in a great many people and done infinite harm to science, from which, as Teall says, they are as far removed as the speculations of the schoolmen, which they to some extent resemble.

Another theory which was supported by great names, including that of Helmholtz, originated with Sutcliffe and Prof. James Thompson (*Glacial Nightmare*, pp. 535-538). This depends on the fact that the freezing point of water is raised by pressure; consequently, if ice at 32° is subjected to sufficient pressure it will melt; and it was argued, to use the clear phrases of Mr. I. C. Russell, that "under pressure, pores occupied by liquid water must instantly be formed in the compressed parts, since ice cannot exist at a temperature of 32° under a pressure exceeding one atmosphere. If the conditions permit, the water thus pressed by the melting of the parts under pressure would be forced to where the pressure was less and at once re-freeze. The parts re-congealed after being melted must in turn, through the yielding of other parts, receive pressure from the applied force, thereby to be again liquefied and to enter again into a similar cycle. In applying this principle to glaciers, it is claimed that the water formed by liquefaction may in part descend, and on re-freezing occupy a lower position. (It might be asked, however, why the water, if under pressure, should descend rather than move in any other direction.)" Mr. Russell further says: "In opposition to this hypothesis it is evident that the greatest pressure at least in the case of a glacier flowing through

an even channel is at the bottom, while the surface sustains the pressure of only one atmosphere, and in the great majority of cases of less than one atmosphere, yet the maximum flow is always at the surface. Additional weight is given to this objection when we recall the fact that the flowing motion observed in glaciers is greatest at the surface, even in cold weather. At such times the surface ice may reasonably be concluded to have a lower temperature than the bottom ice, and therefore requires a greater amount of pressure to cause it to liquefy " (*op. cit.*, pp. 178, 179).

In the *Philosophical Magazine* for 1888 Mr. Deeley propounded what he claimed to be a new theory of glacier motion. In this paper he very rightly says " that every change of outline suffered by a glacier, if we disregard melting and the small internal changes of bulk produced by pressure, etc., is due to a shear of ice plane over ice plane ". He further says of glacier motion : " We have, therefore, two kinds of motion —one a bodily slide in a downward direction, and another due to the differential motion of the ice not in contact with the ground." In order to explain the latter he postulates that constant liquefaction and resolidification is taking place within a glacier by the sun-heat penetrating it and melting certain portions, and inasmuch as a glacier has a tendency to sink in consequence of its gravity, the liquefying of certain portions of its interior will take away the support of the rest and let it sink down.

This theory is, as Le Conte says, a modification of Prof. James Thompson's. It seems to me to be based on a great many unverified premises. In the first place, the notion that sun-heat can penetrate ice and melt small spaces in its interior forming ice flowers (as was shown experimentally by Tyndall) may be true of transparent ice, like lake ice, upon which the experiment was tried, but seems to me extremely improbable when applied to a glacier with its broken, often snow-covered, opaque or opalescent crust and surface. Again, this process of the sun's heat penetrating a glacier and forming occasional and sporadic ice flowers in its midst would not account for the continuous flow of the whole glacier, whatever effect it might have in inducing isolated particles to move.

Thirdly, the theory is based on the notion that glacier

ice when it flows is at the melting point, and in fact Mr. Deeley says it has been experimentally proved that it only moves at this temperature. I altogether traverse the view that anything of the kind has been proved, and the fact that the Alpine, Norwegian and especially the Greenland glaciers all move in the winter shows it to be untenable. Lastly, and this is conclusive, if the theory were right there would not be a continually increasing differential flow from the base of a glacier to its summit and from its sides towards its centre.

All the theories above referred to, however, have now only an historic interest, and are among the failures recorded by the recording angel of science. Necessitated by the supposed validity of Moseley's experiments on the rigidity of ice, they were all swept away when, instead of the question of the shearing of ice being tested in the laboratory with pieces of pond ice or ice artificially made, as Moseley and others had tested it, they were tested by pieces of an actual glacier which had been shown by Hugi and Forel to be of a granular structure. These experiments which I have described in my former work were made successively by Matthews, Bianconi (Tyndall himself), Aitken, Pfaff, Trotter, Mann, and most conclusively by McConnell and Kidd, and showed indisputably that glacier ice is not a rigid body as Moseley had maintained, but that it behaves in the laboratory just as Forbes had maintained it behaves *en masse, viz.*, as a viscous and semifluid substance with all the characteristics of other viscous substances whose fluidity is slight. These experiments were conclusive, I say, and swept away a great deal of ingenious speculation. Those who are interested in the unravelling of scientific truth from the perverse knots into which it is sometimes tied, and of which the history of glacier motion is one of the most remarkable, may find the whole story told in very great fulness and detail in chapter xiii. of the *Glacial Nightmare*, where it occupies seventy-four pages. This detailed treatment was necessary when that work was written, since some of the great geological prophets were still among the unconverted. One of them, as we shall see presently, still remains obdurate.

In 1895 Messrs. Deeley and George Fletcher published a paper in the *Geological Magazine* on the structure of glacier

ice and its bearing upon glacier motion. In this valuable paper the authors pursue the investigation of Forel and others on the granular nature of ice, an account of which I have condensed in my former work. They show that the so-called glacier grains are not all of one size as was concluded by some, and thus prove that Hagenbach was probably right in supposing that they grow at the expense of each other (the big ones eating up the little ones), and they also, I think, proved that the minute structure of the veined or ribboned appearance of glaciers is probably due partly to the arrangement of the crystal grains, partly to a variation in the shape of the grains, and partly to variations in their dimensions. They further point out what we have above referred to, and what is quite true, namely, that a single crystal of ice is only viscous in the direction of the optic axis. A single crystal, or a portion of a crystal, will yield to continuous transverse stress applied in a direction parallel to the optic axis, but will not yield in a direction at right angles to the axis—in brief, viscous shear in such a case may take place in one plane only.

They then proceed to urge that if all the crystalline grains constituting a glacier had their optic axis arranged parallel with the direction of motion, or if there were a large majority of grains so arranged, it would not be difficult to account for the motion of a glacier. But there is not any such relation between the optical structure of the glacier grains and the direction of motion. If we imagine shear to take place in any single grain, the motion will be stopped by adjacent crystals exhibiting rigidity on that plane. Indeed it does not appear that a glacier in moving can make any use of the fact that ice grains are viscous in one plane, for the direction of that plane differs in almost every grain. How, then, they ask, does the glacier move? Why does it as a mass exhibit viscosity? (*Geol. Mag.*, 1895, p. 160.) When Mr. Deeley presently proceeds to answer his own question, he has nothing better to offer us than the theory above discussed and discarded of internal liquefaction and regelation, which seems to me quite untenable. What the explanation and *raison d'être* of the viscosity of glacier ice may be I do not care at present to discuss. It is pleasant for me to express complete concur-

rence in Mr. Deeley's conclusion as contrasted with his theory, which is that " glacier ice behaves like a viscous fluid, and flows from high to low levels much in the same way as does a river of water. . . . Experiments employed to ascertain the conditions of motion of existing glaciers have proved that velocity curves drawn across the glacier are parabolas, distorted somewhat when the ice river is caused to make a bend, much in the same way as are the curves illustrating the flow of a river by the momentum of the water."

" Above Montanvert," he says, " the *mer de glace* is fractured across at one point, and into the crevasses so formed the surface moraine falls and forms unbedded strings of stone and mud. Lower down the melting of the ice at the surface again exposes the débris. But instead of running as a straight line across the glacier, the lines of débris sweep across as great parabolic curves having their apices pointing down stream. No finer illustration of the fact that glacier flow is a strictly viscous phenomenon could be conceived than is here presented " (*ibid.*, pp. 408-415). This, I think, is all admirable, but it would not be thought so everywhere.

Mr. T. C. Chamberlin, a very distinguished and influential American geologist, in his annual address to the Geological Society of America in 1895, discards the viscous theory altogether, and reverts to the notion that ice is a rigid body and to the older explanations of its movement. He admits the granular structure of ice and the continuous growth of the granules, and he suggests that it is by the growth of the granules that the ice movement takes place ; but if the granules merely grow at the expense of each other and by eating each other up, as has been shown by Hagenbach and Deeley, it is hard to see where any extension of the mass of the glacier can occur, and if it did it would grow towards where the resistance was least, that is, it would swell upwards. Chamberlin supplements this notion by a revival of Croll's view that a granule may continually change its form by partial melting and freezing, by loss in one part and gain in another, and through this may either move itself or permit motion in its neighbouring granules or both, it being under the influence of gravity acting directly upon it and also indirectly through surrounding granules. This hypothetical

action of the granules, he argues, would result in a resultant pressure urging motion down the slope. I confess I do not see why it should not swell up the other way, where the resistance was less. Assuredly Ruskin was a wise man when he said that " a great part of the supposed scientific knowledge of the day is simply bad English, and vanishes the moment you translate it" (*Deucalion*, p. 230).

Mr. Chamberlin then continues : " Every warm day sends down into the glacier a wave of heat energy. This enters the upper surface as sensible temperature, but for the most part it is soon changed to potential heat energy in the form of melted ice. We should not fail to see that the sheet of melted ice that creeps down between the granules of the glacier as the result of a day's sun action is as truly a wave of heat energy as if it remained in the form of sensible temperature." This ought to mean something very important for it sounds very profound, but I absolutely fail to see anything whatever like it in what takes place every day on a glacier. A glacier is not a transparent mass of blue ice like a sheet of glass, but where it is not covered by snow it is covered with an almost opaque crust. When the sunshine beats on it that part of the heat which is not reflected into space is very largely absorbed by the surface layers of the glacier and goes to melt, for instance, the two inches of ice which, it has been calculated, is lost from the surface of the Muir glacier on a warm day. That any appreciable amount of sun-heat penetrates the depths of a glacier is, I believe, quite a myth ; at least I know of no evidence for it whatever.

Mr. Chamberlin then goes on to postulate alternations of these meltings in the day with the reversal of the process at night, and similar intermittent processes between summer and winter, and speaks of successive waves of heat energy causing melting where predisposition to melt exists, and freezing where predisposition to freezing exists, in a series of local immeasurable and constantly changing positions in the glacier, and never seems to ask how all this extraordinary proceeding is to be reconciled with the simple observed facts that the differential motion of a glacier is continuously enhanced from its base to its summit and from its flanks towards its centre.

Mr. I. C. Russell, in criticising Chamberlin's view, says that the molecular changes invoked by him cannot take place below 32° unless pressure is greatly increased, and if pressure is the controlling condition then the movements supposed to occur would increase with the depth of ice, and the bottom of a glacier ought to flow more rapidly than its surface, which is the opposite of what takes place, while his statement about waves of energy causing alternate melting and freezing of the ice has not been shown to occur under the most favourable conditions (*op. cit.*, pp. 184, 185).

On the other hand, Chamberlin's objections to the viscous theory seem trivial. He again trots out Tyndall's objection that crevasses prove glacier ice to be brittle rather than viscous. No one has pretended that glacier ice has the fluidity of pitch or honey, but rather that of lava or sealing wax, both of which " crevasse" in precisely the same way when bent, and yet are admittedly viscous.

The same answer may be given to the objection that stones lie on the surface of the ice without descending in it. This is rather an objection against the plasticity of ice, yet who doubts the fact that ice can be squeezed into a mould or that a stone can be pushed into it with sufficient pressure. All that Chamberlin's objection means is that the pressure of the particular stone is not sufficiently great—that is all.

Chamberlin admits the contortions and foldings of the laminations of ice, together with faulting and vein structure, and says they cannot be signs of viscosity since they occur in crystalline rocks. Certainly they do, and my conclusion would be not that ice is not viscous, but that viscosity or something very like it must have existed in these rocks when the similar phenomena were induced in them.

He then offers a theoretic objection to the theory, namely, that ice is a crystalline substance, and that crystalline bodies may readily change their form by the removal of particles from one portion by melting, and their attachment at other points by congelation, but not he thinks by the flowing of crystallised particles over each other while in their crystalline condition. To this Russell replies with effect that if a slab of ice supported at its ends does gradually sag under the influence of its own weight simply, and at temperatures that

do not admit of melting and refreezing, it seems unnecessary to argue that on account of its crystalline structure it is impossible for it so to yield (*op. cit.*, p. 172).

Lastly, Chamberlin says that on two or three of the glaciers he saw it was observed that the surface rose in the direction of the movement of the ice, so that the surface streams flowed backwards. This seems to me to be consistent only with the viscous theory, which in certain places would cause a surge like this in order to overcome an obstacle, and to be inconsistent with any other theory. It seems to me that all Chamberlin's objections to the viscous theory of ice movement fail. While Prof. Chamberlin has reverted to the view that ice is not a viscous but a rigid body and moves according to some transcendental method, the viscous theory is now, I believe, dominant here in Britain.

When I wrote my former work Prof. James Geikie still adhered to Croll's view that ice had some inherent power of motion due to molecular changes within its substance, and which made its movements under certain conditions independent entirely of gravity. This view he has now abandoned, and in the latest edition of *The Great Ice Age* he confesses that (as some of us have always so strongly maintained) Forbes was right in treating ice as a viscous body and its movements as governed by the same conditions and laws which control the movements of other viscous bodies. He says "the physical research of later years has apparently established the truth of Forbes's theory". He then proceeds to state the conditions of ice motion in clear and unexceptionable language. "It is enough," he says, "to be assured that ice under pressure behaves like other solids similarly placed—it flows. As M. Tresca has shown, by many varied experiments, all solids can be made to flow like liquids, and ice is no exception. The latter has only to attain sufficient thickness, and its own weight will suffice to set it in motion. Thus, with its temperature at or near the melting point, ice a few hundred feet in thickness cannot remain inert, even although it lay upon a horizontal surface. It would in such a case flow outward in all directions, until the shearing force came to counterbalance the pressure." This is most true and well said, and we only wish it had

been said years before, as it would have saved some laborious writing.

I will put beside it the emphatic language of the Professor of Geology at Oxford, Prof. Sollas. He says: " The theory of glacier motion propounded by James Forbes now stands alone, undisturbed by conflicting hypotheses. . . . Whatever may be the ultimate explanation, there can be no question of the fact that the ice of glaciers behaves precisely like a plastic solid or highly-viscous liquid, and it is consequently by inquiries into the laws of viscous flow that we may justly seek to extend our knowledge of the movements of flowing ice." That is also well said.

While the fact that glacier ice is viscous is now almost universally admitted, Prof. Chamberlin being the only conspicuous rebel against it, the kind of viscosity which characterises it is still in debate, whether, as Ruskin graphically puts it, it is like honey or treacle or pitch, *i.e.*, having a continuous flow, or like a heap of fresh herrings sliding over each other—a flow of a number of more or less solid pieces slipping over each other.

This question of the particular way in which the viscosity of ice is generated is very interesting, but for us, after all, it is an academic one. The great cardinal matter for us in dissecting the glacial theory is to know that ice acts, so far as we can judge from every experiment that has been tried upon it, as any other viscous substance acts, and, further, that the amount of its viscosity depends very largely on its temperature. The colder it gets the more brittle it becomes, the differential flow of its particles being greatest as we should expect when the ice is nearest to the melting point, and causing it to move faster in the day than at night and in summer than in winter.

The discovery that ice is a viscous body and moves like other viscous bodies was a great blow to the extreme glacial men. Until then it was possible to invoke all kinds of transcendental qualities and powers in ice which were in some occult way buried underneath its very ordinary and everyday appearance, and to invoke monstrous possibilities for it and its capacity. Directly it was shown to be an ordinary viscous body, whose movement like those of other viscous bodies is

due to gravity, those who cared to examine the problem of its movements were obliged to leave cloudland and to come down to the prosaic level where mathematics and physics can be brought to bear upon the problem. They were then necessarily constrained to abandon dreaming and guessing and take to induction.

I am speaking of those inquirers who care for science and scientific methods, not as exercises in dialectics, but as methods for arriving at truth. Those who for years past had wedded themselves to the extravagant hypothesis I have called the Glacial nightmare did not trouble themselves in the least about the consequences of the new and fundamental discovery. So far as I know none of the champions of extreme glacial views, neither Prof. Geikie nor Prof. Chamberlin, nor Dr. Penck nor Dr. Torell, nor any of their myriad scholars, have deigned to inquire whether a viscous body moving under the influence of gravity can as a mere mechanical possibility do the work they impute to it. They merely point to certain facts which are not disputed. They then affirm that those facts are only consistent with the action of ice ; ice, upon whose functions they have neither experimented nor reasoned, whose capacity they have not inquired into or tested — and this is called science. The whole thing to me is a form of exalted sciolism, just as much as fortune-telling or spirit-rapping, or the meteorology of the almanac makers, only that it is put forward under the disguise of scientific terminology.

A solid differs from a liquid in that its particles are not movable among themselves, so that when a solid moves it does so *en masse*, and cannot be poured away or run away gradually. A liquid, on the contrary, is in a condition when its particles can move about among or over each other. In a perfect solid, which is a purely hypothetical thing, no movement whatever is possible under the influence of gravity among its particles. In a perfect liquid, which is also a purely hypothetical thing, the movement of its particles is unimpeded by any friction of any kind *inter se :* they are free to move in any direction *inter se* without any friction or drag of any kind.

Between these two conditions of matter there are what are

known as viscous substances, *i.e.*, substances in which the
particles will move over or past each other under the stress of
a sufficient pressure or force, which is known as the shearing
force. The amount of pressure or force necessary to make the
particles flow over each other or past each other is the
measure of the viscosity of the substance under examination.
The resistance to this flow is due to the friction of the par-
ticles *inter se.*

In a perfect liquid in a state of unstable equilibrium, that is
to say, when one part of it is higher than another, the conse-
quential motion according to theory would be different to that
of a viscous fluid in which there is internal friction. In the
former the movement would be what is called hydrostatic,
that is to say, if the surface of the liquid were uneven the
higher portions would sink and the lower ones would rise
until a level was attained, and this movement would induce a
certain lateral curvilinear movement of particles in the body
of the liquid (increasing as we go down) in the direction of
the point where the lower level of the liquid occurs. This
would be the only movement that would take place in a
perfect liquid. A perfect liquid is, of course, as I have said, a
purely hypothetical fluid.

As a matter of fact all known liquids are viscous, and when
in motion there is some internal friction and some drag of
each particle upon its neighbour. This friction, of course,
interferes with the perfect hydrostatic motion just referred to ;
so that in every known liquid with an uneven surface there
is a double motion induced by gravity : the one just described,
and, secondly, the rolling of the particles over each other from
the higher level to the lower wherever the surface of the
liquid has sufficient slope. As the internal friction is more
and more increased the movement of its particles by rolling over
each other will predominate, and the hydrostatic movement
will diminish until in the case of the more viscous bodies it
will, as I have said, virtually disappear altogether. So long as
the internal friction is not sufficient to counteract the purely
hydrostatic rising and sinking of adjoining columns with a
lateral shift of the lower particles, this movement will go on
to meet the efforts of the liquid to regain equilibrium. This
will, however, become less and less as the liquid becomes more

viscous, until it doubtless disappears altogether in such sub-stances as ice, whose viscosity is very great.

Here I may point out what seems to me a fallacy in my friend Prof. Sollas's ingenious paper in which he has tried to equate the conditions of an ice-sheet with those of a mass of pitch on a flat surface. He mentions how pitch in his experiments sank down at one place and rose at another, showing that it was subject to ordinary hydrostatic move-ments. Ice, so far as we know, is too rigid for this practi-cally to occur, and the only movements in it of the slightest importance are those of its layers as they slide and roll over each other where its surface is sufficiently sloping. This is apparently the reason why the sides of crevasses do not bulge out as we go down, and that we find great cliffs of ice like those of the Antarctic continent and of the tabular icebergs of the South Seas showing no bulging of their lower layers. So far in fact as theory and experiments can lead us, it would seem that the purely hydrostatic movement in ice is reduced virtually to zero. Its motion is limited to the rolling of its particles over each other.

This simplifies the problem considerably. As I have said, the experiments of Forbes prove that ice moves as a viscous substance theoretically should move, that is to say, its upper layers move faster than its lower ones, the increased drag and potency of the friction gradually exhausting the momen-tum of the particles as we descend in the ice mass itself. Tyndall tried some effective experiments which showed this completely. At a point in the *mer de glace* where there was a perpendicular face of ice 150 feet high he drove in three stakes, one at the summit of the ice, another thirty-five feet from the bottom, and a third four feet from the bottom. After twenty-four hours it was found that the top stake had moved forward six inches, the middle one four and a half inches, and the bottom one two and two-third inches. It would seem very probable that even in very large glaciers, except where the slope is sensible, the lowest layers of all, like those of a river, have hardly any residual motion at all, if they have any.

What is true of ice when tested thus perpendicularly is also true of it when tested horizontally, as has been shown

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by numerous experiments, the centre of the ice flow being greater than that of its sides, thus causing the curved lines of structure so visible in most glaciers.

Tyndall differentiated this latter movement with greater precision, and showed that in a glacier as in a river the point of swiftest motion changes from one side of the centre to the other as the flexure of the valley changes, the greatest movement being always on the convex side of the channel. Mr. I. C. Russell states this clearly. He says : " When a glacier follows a sinuous course the thread of maximum current is deflected to the right and left of a medial line, in the same manner that the swift central current of a winding river is thrown first against one bank and then against the other ; but the bends in the sluggish ice current are less abrupt than in the case of the more flexible water ".

The rate at which glaciers move is very variable; it depends on several elements, such as the size of the glacier itself, the slope and rugosity of its bed, and the temperature and consequently the fluidity of the ice. It is greater in the day than in the night, in summer than in winter. (It must be remembered, by the way, that glaciers move in winter as well as in summer.)

In regard to their rate of motion, Tyndall says that most of the great glaciers in the Alps have in summer a central velocity of two feet a day, while there are points on the *mer de glace* opposite the Montanvert which have a daily motion of thirty inches in summer and which in winter have been found to move at half this rate (*op. cit.*, p. 180). The greatest motion noticed by Tyndall in a glacier was thirty-seven inches per day. This was in the centre, and was reduced to about two inches at the sides.

Measurements made by Lieutenant Peary on the flow of the Bowdoin glacier, in about 77°45', showed that the rate during the month of July was four-tenths of a foot at the south-west point, near the east border, and 2·78 feet at the furthest point near the centre, with an average of 1·89 foot for the whole (Russell, *Glaciers in North America*, p. 144 note). Where the glaciers are much larger, however, as in Central Greenland and Alaska, this rate of motion is very largely enhanced. The measurements hitherto made are not quite

consistent with each other in regard to the amount, but they all concur in proving that the amount, as I have said, is many times greater in the latter instances than that of the Alpine glaciers and is many feet a day.

Let us now proceed again. It is a well-known property of viscous bodies that when they are laid upon a flat surface they will begin to move in all directions so long as the slope of their upper surface is sufficient to induce this movement, and the amount of slope thus required is precisely the same as the slope of a bed upon which the same body will begin to flow when its upper and lower layers are parallel. So that it is immaterial whether a glacier is lying on a sloping bed which induces it to move or its upper surface is of the same inclination as the sloping bed in question. In either case the motion will be at the same rate.

In regard to ice there have not, unfortunately, been any very precise experiments to show what slope of the bed is needed to induce motion.

The matter, of course, is complicated by the fact that it is only one of the elements. Its amount is apparently variable, and depends largely on the mass of the glacier. In the Alps, according to Dr. Wright, the lowest mean slopes down which glaciers move are $2\frac{1}{2}°$ to $3°$, or about 250 feet to the mile. In Greenland Jensen found the slope of the Frederikshall glacier to be about seventy-five feet a mile, while Helland found that of the Jacobshavn glacier to be about forty-five feet to the mile, while Croll made his calculations in regard to his great ice-sheets on the supposition that their slope was half a degree, which may perhaps be taken as a fair limit in the case of enormous masses of ice. This means, however, the amount of slope necessary to induce the motion of the surface layers only, and, as we have seen, this motion must diminish continually as we get nearer the bottom, until in ordinary glaciers it becomes almost nil. It is, however, I know, useless to quote glaciers to the ice men. They repudiate glaciers as tests altogether, just as they repudiate laboratory experiments upon ice. With them all inductive methods and arguments fail, since they always reply that the ice they appeal to is something entirely different to the ice of glaciers. It is ice-sheets they rely upon, portentous ice-sheets, such as no

longer exist anywhere. A Saturnian postulate, in fact, is their platform, and not a mundane one. Yet it ought to be a condition even of such a transcendental postulate as this that the ice in an ice-sheet should act in accordance with, and not contrary to, the nature and the physical qualities of ice. If it do not, the appeal ceases to be a scientific appeal, and it is, in fact, very largely an unscientific appeal which is continually being made by this noisy, clamorous school of writers, who never verify their premises and make assumptions as readily as they abandon them.

An ice-sheet is only a great mass of ice after all ; a mass of ice which, instead of lying on a mountain slope or being embayed in a valley or on a plane surface, is supposed to have smothered and covered a stretch of uneven country and swathed it in a continuous mantle. Such a mass of ice cannot acquire properties not possessed by other ice. If it moves it must move according to the mechanics of ice, and, as we have seen, ice moves in no other fashion than by the influence of gravity.

In an ice-sheet, which is theoretically a mound of ice piled on a surface which is not sloping, the only movement possible must be induced by its having a sufficient slope along its back to induce such motion. The height of the necessary mound, if we are to have an adequate slope, needed by the glacial theory of most authors may be measured by the fact that both in Europe and America the ice-sheets have been supposed to carry débris for 600 to 1,000 miles ! ! ! They must, therefore, have had that radius in order to induce movement in the surface layers of the ice. In order that an ice mound of this radius should move at all, its aggregate slope must have been very great. This means, in fact, that it was an ice dome many miles high, which must necessarily have crushed and been destroyed by its own weight, the nether layers being reduced to mere slush. But what kind of Brobdingnagian ice mound are we to invoke if we are to move not merely the surface but the nether layers of the same ice-sheets over the same distances ? This last condition is an absolute necessity if we are to explain the distribution of the drift as the ice men explain it. It must be remembered that the deeper the mound the greater the internal friction as we descend into it. It really requires the imagination of Swift to realise

the tremendous ice mountain that would be needed to move the bottom layers of ice over 600 miles of level or broken country. The whole thing is merely a bad dream. How is such a mound to be formed? Where are the meteorological conditions to be found? Why should it be formed in the comparatively low grounds of Sweden and Labrador? Why should the ice culminate in this way at all in any particular district, and gradually die out in every other direction? When people believed in polar ice caps there was a certain *raison d'être* in the suggestion. It was plausible to suppose that the two polar areas were probably the culminating points of the earth's snowfall, and that as we departed from the poles we generally left the area of excessive snow. But no one believes in polar ice caps now; they are as dead as Queen Anne; and we now know that whatever distributed the drift distributed it towards the pole as well as sporadically in other directions. How, then, are we to account for the culmination of ice-sheets, not over the polar areas, but on such unexpected districts as Scandinavia and Labrador? What meteorological conditions unknown to us must have existed to cause a snowfall continuously there greater than elsewhere with such a condition of things in these areas that the ice which grew on them was sufficiently powerful to drive stones and other débris even towards the polar areas? No one has tried to answer the question, which, I believe, to be unanswerable. There are no gigantic congeries of mountains and no other physical features there naturally marking them out as the gathering ground of snow on a great scale. On the contrary, both in Labrador and in Sweden (whence, and not from Norway, most of the far-travelled drift travelled) the mountains are comparatively low, and both countries are remarkably dry instead of being the natural converging points of damp, snow-laden winds.

How, again, under the conditions postulated by the glacialists, when half the northern hemisphere down to quite temperate latitudes is supposed to have been covered with ice, could these ice-sheets be formed at all? If the cold was so intense as is made out, it is difficult to understand how there could be rain and moisture, and without rain snow does not form into glacial ice, *but remains dry snow or névé.* It is below the snow

line and not above it that the glaciers are chiefly formed, and in so-called glacial times the snow line must have been at the sea level.

But granting the possibility of getting the ice and thus piling it up, unless the ice-mound started into existence all at once, how is it possible to understand how there could be anything but very transient and slight local movements in the merest surface layers of the ice-sheet? Directly the slope of equilibrium in the ice was attained a fresh snowfall would cause a very slight excess of slope, which would not go on accumulating but would begin at once to dissipate itself on the surface layers, and there would be no resulting force left to be carried down through the postulated miles-deep of accumulated ice. In every way, therefore, we view these ice-sheets, the impossibility of their very existence, apart from their doing the work demanded from them, is patent. When we are pointed to Greenland the comparison is quite inept and inappropriate; we are pointed, in fact, to a great number of glorified glaciers and not to an ice-sheet. Greenland nurses on its summit a great *mer de glace*, that is true, and this *mer de glace* pours itself out of its fiords and fissures by very rapidly descending paths; but these streams of ice are all true glaciers, and there is no other difference between them and the Alpine glaciers than mere size and rapidity of movement induced by greater momentum. If we chip holes out of the rim of a tea-cup and fill the tea-cup to the brim with treacle we shall have a miniature copy of Greenland with its central *mer de glace* and its fiords. The treacle will pour out of the chipped holes as the ice does from its fissures.

In view of the theoretical difficulties here mentioned—which are very elementary ones—it may well be asked how it comes about that the champions of the glacial nightmare should so jauntily postulate that ice in their ice age actually did travel over hundreds of miles in flat countries, over the plains of Poland and the northern prairies of America. The answer is plain: they have simply ignored these and all other physical difficulties, as is their custom.

For them it is enough that a large number of stones (partly angular), which must have come from northern Sweden and Finland (since the beds of rock from which they are detached

are only found in those countries), have certainly travelled by some means or other as far as the Carpathians and the latitudes of Central Russia. This fact no one disputes ; what I most emphatically do dispute is that ice did or could so carry them. I have pointed out the difficulties from the point of view of the physical qualities of ice ; let me now say a few words in regard to the stones. If we are to judge from any of the glaciers which we can examine, the only stones which they carry are those which have originally fallen upon their backs from the crags and other projecting rocks which rise above the ice. The Gorner Grat is a good place from which to observe the whole process. The back of the great glacier which flows below is marked by lines of black débris running parallel to its sides, otherwise the ice surface is clean and free from stones. If we follow these traînées to their source we shall find that they originate in stones or showers of stones detached from disintegrating rocks which rise in the middle of the glacier, or which bound it on either side, and which it carries along with it in its progress. But for the existence of crevasses this débris would be carried right down to the foot of the glacier. When the stones, however, reach a crevasse they naturally fall into it, and if the crevasse reaches the bottom they fall to the bottom. There their angles are rounded and they are smoothed and scratched by being pressed against the bed of the glacier or by being rolled in the glacier streams. They travel down to the foot of the glacier, where they are presently joined by those of their companions which have escaped the crevasses, and, having travelled on the glacier's back, have retained their angular outlines. The two classes of stones are found mixed together in the moraines at the feet of glaciers. This is all perfectly true and simple, but how is it to be applied to the explanation of the far-travelled stones of the so-called glacial age ?

As we have seen, in order that an ice-sheet should extend from Dalecarlia to the Carpathians, or from the Christiania fiord to Cromer in Norfolk, and carry stones on its back all the way, it must have had such a slope in its upper surface that every mountain top in Scandinavia would be smothered deep in ice. If so, how and whence could any stones at all have got on to the glacier's back, especially the particular

stones in question, which have come not from the mountains but from the lowlands of Sweden and Norway ? This is surely a stupendous difficulty to add to those that I have already deduced from the conditions of movement of the ice itself.

This is not a fantastic objection. If we go to Greenland we shall find that where there are no " nunatakker " (as the rocks projecting above the ice are called) there then are no moraines.

So much for the stones travelling on the glacier's back; but we have to account not only for these, the angular stones, which are only a small proportion, but for the rounded ones, which must have travelled under the ice-sheet if the ice took them at all. To explain these the older glacialists invented the theory of what they called " ground moraines," according to which the ice-sheet was underlaid by a mixed mass of sand, clay and boulders, which it dragged along beneath it as a boy drags his trousers when he is slipping down an ice slope on which he is seated. I have analysed the notion of ground moraines at considerable length in my former work, to which I must refer (*Glacial Nightmare*, pp. 689-693).

The difficulty of understanding such a postulate is absolutely portentous. First, as to the origin of the stones. So far as we know, crevasses only occur when the ice is travelling down sharp slopes or is going over hummocky ground. But in the vast ice-sheets, which are postulated as having occurred in the so-called ice age, there could be no crevasses at all over a great part of their route, for it was a level plain, while the ice would be so deep in the more sloping mountain districts whence it came that we cannot understand how the crevasses could reach the bed at all. Even therefore if there were any exposed crags to supply stones, which we have shown to be virtually impossible, these stones could never reach the base of the ice-sheet. But it must be remembered that the travelled stones not only come from the far-off country whence the ice-sheets are supposed to have started, but they must have been picked up all the way, for they represent all the rocks in the intervening country. Suppose, however, we could get the stones, how could they be made into boulders, as so many of them have been made? If the ice rubbed them against the nether rock, it would make them into slipper stones with parallel

faces, or cause them to be faceted as such glacier stones are now found to be.

The true sub-glacier boulders which are now being made—that is, the rounded stones—are being rolled by the sub-glacier streams. But how could there be sub-glacier streams in the ice age? The postulated conditions of that age are such as are not matched by the winter temperature of any part of the globe now. If it is possible to understand the melting of the ice-sheet along its borders, we cannot understand the presence of running water in the far-off country whence the stones actually came from—that is, where the ice and the cold culminated—nor can we understand how, if there was water, it could run up and down hill, into hollows and valleys, and up again. At least water cannot behave in this fashion now. Again, how could *streams* or *rivers*, sub-glacial or otherwise, deposit moraines with several hundred miles of unbroken front unless the whole ice-sheet was floating on water, the result of its own melting, and this at a time when the most desperate cold prevailed everywhere?

Still one more hypothetical possibility. The glacialists do not for the most part claim that the drift was distributed by the sub-glacial rivers, but that it was dragged along by the ice itself. They do not trouble about how the boulders in such a case were made, or the other difficulties I have pointed out. To them the only problem is the carrying of the stones, and they profess to argue, as I have said, that the ice drags them along with it. We have already seen that in an ordinary glacier the bottom layers of the ice hardly move at all, and even the upper ones cease to move soon after it has reached level ground. How, under these circumstances, we are to give sufficient force to the lowest layers of an ice-sheet to enable them not only to move hundreds of miles but to drag stones underneath it and along with it I do not know. It is not merely stones, however, that have to be accounted for, but great masses of clay and sand. How ice could drag these materials along beneath it unless they were frozen hard I do not know, for the particles would slip over each other and no progressive movement would ensue; and if they were so frozen the whole would be mixed into sludge and what the Americans call "muck," and not be sorted out beautifully as we see the clays and sands of

the drift sorted; in many cases the latter being cross-bedded and marked by laminar structure. It does not affect the glacial champions to be told by Bonney and others that nothing like the boulder clays or the drifts as we know them is being made by glaciers or is found in their abandoned beds. Nor does it apparently affect them to be told, again, that in many cases when modern glaciers get on level ground they pass over soft beds without disturbing them or moving them. These are arguments drawn from induction, and the modern geologist despises induction.

Let us proceed, however. It is not merely ice moving and acting as a porter over a slope or a flat surface which has to be explained. The great virtue of ice-sheets in the eyes of their champions is that they are quite independent of the ordinary laws of gravity, and able to move up and down hill and across a broken country.

The extent to which an appeal has been made to this transcendental movement of ice into deep hollows and out again, dragging along with it great masses of débris, not merely big boulders but gravel and sand, is quite astounding. For example, in one well-known instance, such a ground moraine has been taken over the very irregular surface of Sweden, across the deep hollow of the Baltic, then up again and across the great plains of Germany and Poland. On another side it has been taken across the very deep channel that runs down the west coast of Norway, and up again, and then across the uneven bed of the North Sea, marked by deep hollows and shallow banks. Elsewhere, again, it has been taken down into the depths of the lake of Geneva, which it is supposed to have scoured of its contents, then moved up again into the Jura Mountains. This movement up and down hill of great masses of ice is quite incomprehensible to me.

There can be no doubt of the capacity of ice to travel up-hill within certain narrow limits. If the force of gravitation which moves its layers be sufficient, it can no doubt be pushed or made to flow over hummocky ground, or up a slope of a certain limited character just as well as over level ground. The amount of the upward march it can thus make is a function dependent upon certain ascertainable quantities and can be calculated, and the very same argument applies to ice-sheets.

It is a purely mechanical problem dependent upon the gravitating force available and the obstacle to be overcome, just as the motion of a train up a gradient is, and it is speedily exhausted. Because, however, a certain movement of this kind is possible with a viscous substance it is apparently supposed by the glacial men that any amount of such movement is possible. The fact that the residual pressure at the foot of a long glacier is only sufficient to push it a short distance along a flat surface shows how very limited this capacity must become when the gravity to be overcome involves travelling uphill. In all cases of such movements known to me the ice mass has simply been made to move over a hump or boss in its bed. It must further be remembered that the expenditure of force in overcoming one such obstacle will *pro tanto* exhaust the capacity of the glacier for overcoming further ones, just as a man who has one go-cart to push will find it more difficult to push two.

That a glacier has some difficulty in overcoming such obstacles as humps in its bed, even when its motion is helped by its being on a sloping channel, is proved by the existence of crevasses. Where the slope is even, or where the ice is travelling on level ground, there are no crevasses. It is only when the ice has to make a bend over some hump or mound in its course that the strain upon it becomes too great for the viscous flow to quite overcome it, and it cracks and then gapes into a V-shaped fissure, the upper layers below the crack flowing faster than those above it. When the obstacle is passed, and when the upper part of the glacier has more momentum from its larger or faster flow, the fissure or crevasse fills up again. The same takes place when the glacier turns round a sharp curve, and when a similar strain is put upon the concave surface of the mass. Ice which thus cracks and gapes with the slight strain of travelling over hummocky ground is nevertheless supposed to have travelled intact right athwart the drainage of the country, over ridges of mountains, and then to have climbed up great slopes thousands of feet in height, and this too directly after emerging from a journey down into a great trough and up again. I refer, as mere examples, to the Swedish ice-sheet, which is supposed to have travelled westwards, and, coming from comparatively low ground, to

ice or Water.

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have overwhelmed the much higher Dovre Fjeld ; the Irish Sea glacier, which, it is postulated, climbed the considerable mountain of Barule, in the Isle of Man, and up the lower flanks of Snowdon ; and the Cumberland glacier, which is made to climb the English Apennines, into and across the plains of Yorkshire. I propose to discuss and criticise such instances at a later stage and in more detail. I merely mention them here as samples of the load which the galled jade, ice, has been called upon to carry without wincing, and as instances of the mechanical difficulties habitually evaded by the glacialists, and of the impossible tasks they assign to the handiwork of ice.

The reason for postulating these movements of the ice uphill is the finding of great angular erratics on heights far above where the rock whence they were detached is found *in situ.* ' These angular erratics must have travelled (if ice carried them) on the ice's back, since they are not in many cases rolled or rubbed. But how could they get on the ice's back ? When the ice was travelling over heights several hundred feet over the bed-rock, it must have buried that bed-rock deeper down still below its travelling mass. The ice must have culminated over the bed-rocks and sloped towards where the erratics are formed, for we cannot understand how the layers of a viscous substance, any more than those of a liquid, can travel uphill for long distances with no other impulse than their own gravity. All this is surely adding phantasm to phantasm.

It is not merely the tremendous journeys uphill which glaciers are supposed to have taken in the face of gravity which it is so difficult to explain. They are also supposed to have gone down into deep hollows with steep sides, like the Baltic and the deep channel off the coast of Norway, and athwart the lake of Geneva, and then to have climbed up on the other side. When once the ice entered such troughs it would be entrapped and embayed there. Any movement that then occurred would only be in the upper layers, which might perhaps flow over the embayed ice if its own surface slope was sufficient, and it would have no more effect on the layers at the bottom than the waves in the North Sea have upon the water in the Silver pits. In order to drag the nether layers of the ice up out of the deep hollows in question

we should require such an inclination in the ice slope as is quite incredible, unless we postulate that a vast ice mound with steep sides was suddenly made, for, as we have argued, any slowly growing ice mass would dissipate its slopes as quickly as they were reinforced by fresh materials.

It is not my view only that ice is incompetent to perform this work; the view is shared by much better men than I am. I have put it before some of the very acutest and most experienced physicists in these realms, and in every case I have received the reply that the whole thing is simply absurd. I have never met a physicist who has not laughed the notion to scorn. It is, in fact, a mere philosophic nightmare; the kind of thing men see in dreams, but not in reality, and is only possible because the modern geologist (except in very few cases) knows very little of mathematics or physics or the laws of matter.

I should like to cite a concrete example of the mode of reasoning outside of ordinary logic sometimes employed in current geology, and will quote from a geologist who has done good work in the field. The view I refer to is that of Mr. Goodchild, and it is to the effect that in a glacier moving down a slope, or in a dome of ice or ice-sheet spreading in consequence of its own sloped surface, there can be currents at various depths in the ice in various directions and even athwart each other. This theory seems to ignore every consideration of hydrodynamics and of the laws of gravity. If two glaciers meet at an angle their ice streams may flow side by side. If the angle is not sharp one may thrust the other back, and if its force be greatly bigger than its rival's it may override it for a space, just as may happen with contending currents of water. No doubt, again, when a glacier is coming down a valley with a very gentle slope and a slight momentum, and is joined by a subsidiary glacier from an influent valley (which may be steeper), the intruding glacier, whose flow is more rapid, may thrust the other glacier back for a short space, and it will appear as if the ice were moving in two directions, and this will continue so till the two ice streams have coalesced. But this, which is a perfectly simple and credible mechanical process, and only involves the adjustment of two rivers of ice with initially varying force and direction when accommodating themselves to a common path,

is a very different process to divergent currents arising or continuing in ice-sheets, and being continued in them when the ice is supposed to be moving across natural obstacles. That in a continuous river of ice flowing down an inclined bed by reason of gravity, or in a mound of ice assuming a position of equilibrium by the ice layers rolling over each other, there can be rival and transverse and divergent and cross currents seems to be as impossible as to suppose that water running down a smooth channel can have transverse and cross currents at different depths, and can travel at varying angles with the lines of its own gravitating motion. I have sufficiently criticised this preposterous view in my former work (*Glacial Nightmare*, pp. 679-681).

Mr. Goodchild does not stand alone. The stress to which the glacialist champions have been put has led them, especially in America, to adopt similar modes of argument.

Thus Mr. W. O. Crosby says: "Transportation of drift by simple drag is relatively unimportant if not impossible. The transportation is almost entirely englacial, but highly differential, being extremely slow in the basal layers and more and more rapid at higher levels" (*Geol. Mag.*, 1897, p. 323). This by the way looks like giving up the ground-moraine theory.

Professor Upham also argues that the greater part of the drift was not transported as a ground moraine, but embedded in the lower layers of the ice-sheet.

It is perfectly familiar to those who have examined glaciers that besides the stones on their backs and the stones underneath them there occur in some cases stones of various shapes and sizes and sometimes portions of dirt and gravel embedded in the mass of the ice itself. I have seen such occasionally in the walls of the so-called ice caves. This enclosed débris is what the Americans call "englacial drift," that name having been given to it by Chamberlin. It is a phrase which occurs very frequently in their recent glacial literature. This englacial drift is supposed to travel up through the glacier irrespective of gravity in a most curious fashion, and to be thus able to explain some paradoxes.

What are the facts? When a mass of rock tumbles down from an exposed crag on to a glacier, if it fall on to the

upper or snowy part of it, it will bury itself in the snow, and the next snowstorm will cover it in; if it falls on the névé, or on the upper ice, it will be similarly covered by subsequent snowfalls. As the snow and the névé are converted into blue ice these stones will of course be embedded in the ice itself, and this will sometimes happen also with portions of gravel and dirt sliding down from the flanks of the valley.

Again, as we have seen, it is very probable that most crevasses do not reach down to the beds of the glaciers. They are merely gaping cracks opening where the ice is strained and closing up again when the strain is removed. It is perfectly plain that when stones fall into such crevasses they will also be entrapped in the middle of the blue ice. This is a simple and reasonable explanation of englacial drift. As the glacier moves down to lower and warmer altitudes the melting and ablation of its surface naturally increase. Hence in many cases the stones which have been encased in the blue ice appear at the surface and look as if they had been discharged from the bowels of the glacier itself. This, again, is a simple and reasonable explanation of an everyday occurrence.

Thirdly, the gradual ablation of layers in a glacier causes the lowest layers in it to be thinner than the upper ones, and this condensation causes the stony débris they contain to be greater in quantity in the lower parts of a glacier than in the upper ones—a simple fact for which some recondite reasons have been unnecessarily adduced. These direct and simple explanations dispense altogether with the fantastic notion which has been urged by many that in a viscous fluid, in which the layers flow over each other by gravity alone and in which the stream lines must be direct, the enclosed stones and other débris can travel upwards in the teeth of the flow of the ice, *i.e.*, from layers where the motion is slight and the restraining pressure is great to layers where the motion is greater and the pressure less. Of course, when the layers of a glacier adopt a meandering route in travelling over a hummocky or meandering bed they will carry the stones with them, and occasionally when a hump is in the way this may cause a dragging of the stones slightly over the adjoining ice stream. But this is a very slight and occasional

incident, and in most cases is probably counterbalanced in effect by the crevasses caused by the humps. Mr. Deeley's notion (*Geol. Mag.*, 1898, pp. 564, 565) that a glacier may in some cases adhere to its base and in others be frozen to it is very hard to understand. I fail to see any possible cause that would operate in this way, not on a slope with embayed hollows as he figures it, but on a huge flat plain such as the postulated ice-sheets must have traversed if they carried the drift.

The cases quoted above from Mr. Spencer and others in regard to the way in which the stones cut great rifts in the ice which moves over them show how futile this notion is. To them I may add what Mr. W. O. Crosby says: "The Greenland studies of Prof. Chamberlin show the facility with which ice shears along innumerable lines of débris. It slides over the débris instead of dragging it along as it would if the débris were firmly frozen into the ice" (*Geol. Mag.*, 1897, p. 322).

Professor Sollas's experiments upon pitch glaciers in miniature do not seem to me to be very illuminating on the subject, for in them the grains of starch were carried upwards, not by the viscous flow, but by the hydrostatic adjustment of levels in the pitch, a process which we have seen to be quite out of the question in the case of ice. So much for the modes and methods of glaciers when acting as porters.

Let us now turn to the erosive and denuding tendencies which have been attributed to ice. Here, again, I am only making more complete what I have already tried to establish at great length in my former work. Ice, like water, acts as an eroder in two ways. In the first place, when merely ice, that is, when unloaded with stones and other sharp and hard denuding tools, and, secondly, when so armed.

Like water, the eroding tendency of ice when unarmed is, so far as we can judge, both from *a priori* considerations and from experience, very slight, and is limited to smoothing its bed and removing any asperities it may present; to, in fact, removing salient points and angles. It is a mere burnisher and polisher, and this in a very slow fashion indeed, for the layers of the ice in contact with the bed and walls of glaciers are, as we have seen, almost quiescent.

When ice unloaded with stones first enters a valley with

rough and torn edges to its rocks it proceeds, no doubt, to rub these rough places down gently and slowly like a river of water does, and to smooth them and to remove the parts that cause friction until it creates for itself a smooth and polished bed like that of a river. The result of this action is the pouring out at its foot of a considerable quantity of mealy water. This part of its work is, however, presently finished, and except where the slope is great and where the ice probably moves *en masse* it ceases to denude altogether, having made for itself a virtually frictionless route to travel over, just as a river only denudes in its torrential parts. This smoothing and polishing of its bed, which is sometimes gently sloping and sometimes undulating and hummocky, may be seen wherever a tunnel gives access to the interior economy of a glacier or where a glacier has recently retired. The only condition necessary is that the ice shall be ice in motion, that is to say, shall be a glacier. Where ice is embayed and has ceased to move, no such polishing or abrading would be possible. So much for ice when unloaded with stones, etc. Let us now see what happens when it is so loaded.

When a quantity of sharp-edged sand gets in between the ice and the wall of rock along which it is travelling the sand will in certain cases scratch the polished stone, and to this the scratches on the side walls of many Alpine valleys have been attributed. It must be said however in this behalf that unless the subsequent subaerial erosion of the walls was very slight it is singular that these fine lines should have been preserved as they have been. Nor must the possibilities of such action be exaggerated. It is difficult indeed to understand how sand could be held sufficiently tightly in the grip of such a yielding substance as ice, with a motion so very slow, to enable it to act like a rasp or like emery paper. The sand would, it seems to me, squeeze itself into the yielding ice under pressure rather than scratch the hard, smooth crystalline surface of the bed-rock. The latter process may occur sometimes or exceptionally, but it is not easy to understand.

I may say that my scepticism in regard to such ice action is no less aroused by the numerous fine striæ which have so often been pointed out upon the boulders of the drift, and which have been produced as irrefragable proofs of an ice age.

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Striated stones no doubt do occur in true moraines, but they are much fewer than would be thought by those who have not seen glaciers at home. Nor is it quite easy to see how they could become scratched under the normal conditions which dominate a glacier. We have seen with what a tender and yielding grasp stones, etc., are held by a glacier. If such stones are angular and rugose they may no doubt, notwithstanding such yielding, striate the bottom rock or sides of a glacier occasionally. What however puzzles me is how the polished and smooth bed of the glacier or ice-sheet is to scratch and scour the loose stones in the ice—that is to say, how the polished brass plate is to scratch the emery paper, or how two polished brass plates are to scratch each other. How a smooth surface can scratch anything I do not know.

Occasionally two loose stones under a glacier (each partially rounded only) might rub against and might scratch each other; but this must be a very uncommon performance, as is proved by the rarity of scratched stones in true living moraines—the workshops which the genuine glacialist carefully avoids. It has been said that grains of sand might do it; but grains of sand are even more difficult for a glacier to hold tightly than considerable stones, and in all cases we must, as Ruskin reminds us, remember the extreme slowness of the ice motion.

The great majority of the stones from the Eastern counties on which I have seen scratches are the native soft rocks. The number of hard crystalline boulders of foreign origin on which I have noticed scratches is very small indeed. In all cases the scratches have occurred on stones more or less rolled, and in the case of the polygonal chalk boulders not only on rolled boulders but on boulders with polished facets. These scratches were certainly made contemporaneously with or subsequently to the rolling which destroyed the angles and polished the surface of the stones. If so, how could ice be the agent which produced them? We cannot well postulate an ice-sheet in Norfolk and Suffolk dragging in to itself rounded and polished stones from its own sub-glacial streams or from the open waters of the North Sea, where the boulders were being rolled and smoothed, in order to give them a few finishing touches. The process seems absurd. The fact is

these scratched stones have become a fetish. The glacialist naturally wants to find, if he can, a simple and ready test of the work of ice, and he has jumped at this particular test as a very convenient one, when as a matter of fact it is no test at all. The very fact of there being a considerable number of scratched stones in a bed of clay or sand is a proof that that bed is unlike a moraine, and not that it is like it. The many bruised faces and black eyes which used to be seen in Clare Market were not, as some glacialists would argue, the results of the battle of Waterloo—where there were many wounds inflicted, but not many of that kind.

While we are discussing the supposed ice scratches I must say a passing word about a phenomenon I must refer to again, which has also been a sheet-anchor to those who believe in ice-sheets, *viz.*, the existence of striæ crossing a large district in straight lines quite independently of its contour, and doing this for many miles. How this could possibly happen with loaded ice I cannot conceive. The very fact of the ice holding the stones with such a gentle grasp makes it impossible to suppose that it could exercise such a pressure upon them as to make them engrave straight lines even if the country were level, but when the country is rolling and uneven the ice layers if they moved at all must have moved not in straight lines but in undulating ones, and so must the tools in its grasp. How then an ice-sheet coming from Dalecarlia could march right across the Dovre Fjeld and score its path with straight lines miles in length is to me a mechanical mystery.

Let us follow the process that goes on in a glacier. A stone falls to the bottom of a crevasse. The ice begins to move, and the stone is rapidly enveloped in it, and is held in its grasp as in a vice. As the ice moves on and exercises a certain thrust upon the stone it will act like the slipper or skid of a coach, and its lower surface in contact with the glacier's bed will be worn down and smoothed and polished. If presently the stone be arrested by some obstacle, and the ice flows over it, it will in some measure also rub down the rugosities of the upper surface, but probably not to the same extent. The stone will thus have two more or less parallel faces—one a good deal smoothed, and the other not quite so much—but the other angles or edges of the stone will remain intact and

unrounded and unsmoothed. We cannot understand how by any process a glacier can make a true boulder with sub-angular edges or a curved outline all round, or in the case of soft polygonal stones polish and smooth the different facets as well as the angles. This rounding and smoothing and blunting of angles have always been attributed to water, and I take it that this conclusion is generally held still. The only true boulders connected with glaciers are, in fact, the result of rolling in subglacial streams.

Let us now turn to the actual eroding work done by glaciers. When angular rubble or blocks of stone fall, first on the glacier, and then down its crevasses, and sometimes between the glacier and its walls, they become rounded and worn (very largely in consequence of being rolled in the sub-glacial streams); but in addition to this they no doubt cause some erosion and some cutting down of the surfaces against which they rest, that is if they press against them with sufficient force and if the motion is sustained. This, however, has been enormously exaggerated. The eroding work of glaciers has been, in fact, measured by quite untenable tests.

First, in regard to the milky colour of glacier streams, which has been supposed to measure the amount of erosion which a glacier practises upon its bed. According to Heim, the milky colour of glacier streams arises from the *fine division* of the particles held in suspension by them, and not from their quantity. The solid matter brought down by a glacier stream in a year has been found by experiment to be far less than that brought down by a stream draining an area of similar extent not glaciated (Freshfield, *Proceedings of the Geog. Soc.*, 1888, p. 789).

This is not all. As I have urged elsewhere, the eroded matter which gives the glacier streams their creamy colour comes only in a very small degree from the erosion of the glacier's bed. The point has been well put by my friend the late Principal Dawson. " Many observers," he says, "have taken it for granted that the mud sent off from glaciers, and which is so much greater in amount than the matter remaining in their moraines, must be ground from the bottom of the glacier valleys, and hence have attributed to glaciers great power of cutting out and deepening their valleys. But

this is evidently an error, just as it would be an error to suppose the flour of a grist-mill ground out of the mill stones. Glaciers, it is true, groove and striate and polish the rocks over which they move, and especially wear the projecting points and slight elevations in their beds; but the material which they grind up is principally derived from the exposed frost-bitten rocks above them, and the rocky floor under the glacier is merely the nether millstone against which these loose stones are crushed. The glaciers, in short, can scarcely be regarded as cutting agents at all in so far as the sides and bottoms of their beds are concerned " (*Salient Points in the Science of the Earth*, pp. 357-360).

A similar argument was used long ago by Lyell in his polemic with Ramsay. He says : " It is possible to overrate the amount of denudation implied by this muddy overflow. It must not be forgotten that the rocky fragments which are showered down on a glacier from the hillsides above and from lateral and medial moraines must to a considerable extent fall through crevasses, and so reach the bottom of the glacier. It is these masses which, by their friction on the underlying floor, produce the flour of rock ; and there can be little doubt that from being already decomposed by rain and frost they will suffer more in the crushing and grinding process than will the rocky floor which has long been worn down to a smooth surface and is not exposed to atmospheric changes. A large share, therefore, of the detritus issuing from the foot of an ordinary glacier must be derived from the transported fragments, and cannot be adduced as proof of erosion " (*Antiquity of Man*, pp. 357, 358). In other words, a large part of the meal made by the glacier mill comes very largely from the corn that is being ground, *i.e.*, from the loose débris and not from the millstones—the base and sides of the glacier.

Let us continue, however. In order to do efficient eroding work upon a hard or crystalline valley bottom, it is not enough that we should have gravel and other stones to act as chisels ; it is also necessary that the stony chisels should be firmly and tightly held. Now, there is a great deal of misconception on this subject among geologists who have not seen a glacier. They fancy that the stones are held tightly, as in a vice or a lathe,

and they argue, further, as if the weight of the glacier causes
the stone to press with enormous force on the bed. This would
be so if the ice were a rigid body instead of a plastic one softer
than its bed. The fact is that the pressure causes the stone
to force itself into the ice rather than against the stony bed
on which the glacier travels or against its sides. I must here
quote a paragraph or two from some keen observers of glacial
mechanics. First, I will turn to some important observations
of Prof. Niles, which I only referred to in my former work.

On a visit to the Aletsch glacier in 1878 he tells us he
had an excellent opportunity of examining its under side
in front, and observed numerous elongated ridges of
rock over which the ice was flowing lengthwise, adjusting
itself to all the corrugated surface. When the ice passed the
lee end of the ridge it carried with it " the mould of the profile
so perfectly that for more than twenty feet the blue arch
presented a series of parallel furrows, like the flutings of a
Doric column ".

" There was at that time another highly interesting and
instructive exhibition of glacier action. Within a few feet
of the down stream and of one of these elongated *roches
moutonnées* and upon its crest there was a boulder fully three
feet in diameter which evidently had been moving along
this ridge for some distance, probably from its upper end.
There were two sides of the block of stone which were not
encased in ice, *viz.*, the lower one resting upon the rock and
the one facing down the glacier. From the lower end of the
ridge of rock I looked at the boulder through a tunnel of
pure blue ice, which was continued as a deep furrow in the
under surface of the glacier for fully thirty feet from its
beginning. As this was produced by the ice moving over
and beyond the boulder it was evident that the ice was
moving more rapidly than the stone. I afterwards found
other examples of the same kind. . . . It will be understood
that these stones were sufficiently below the upper surface
of the glacier to be removed from the effects of the ordinary
changes of the temperature of the atmosphere. Although
stones which are exposed to such changes may be frozen into
the ice at the edges of the glacier, yet I believe these were so
situated as to correctly represent the conditions and move-

ments of this at still greater depths. If this is correct, and I believe it is, it follows that such fragments of rock are not rigidly held in fixed positions in the under surface of glaciers and carried along irresistibly at the same rate, but that the constantly melting ice actually flows over them, and that their motion is one of extreme slowness, even when compared with the motion of the glacier itself" (*Amer. Journ. of Science*, cxvi., p. 366, etc.).

Prof. Spencer has made similar observations in Norway, to which I also referred in my former work. *Inter alia*, he says of certain stones he saw under glaciers there: "Although held in the ice on four sides with a force pushing downwards, the viscosity of the ice or the resistance of its molecules in disengaging themselves from each other in order to flow was less than that of the friction between the loose stones and the rock; consequently, the ice flowed around and over the stones, leaving long grooves upon the under surfaces of the glacier" (*American Naturalist*, xxii., p. 218, etc.).

This shows what inefficient eroding tools stones are when held in the yielding, waxy hands of a glacier, and how limited their power must be.

I will in this behalf also quote a passage from Ruskin, whose acute observations in the Alps have not been sufficiently appreciated. "A stone at the bottom of a stream or deep sea current necessarily and always presses on the bottom with the weight of the column of water above it, plus the excess of its own weight above that of a bulk of water equal to its own; but a stone under a glacier may be hitched or suspended in the ice itself for long spaces, not touching bottom at all. When dropped at last, the weight of the ice may not come upon it for years, for that weight is only carried on certain spaces of the rock bed; and in these very spaces the utmost a stone can do is to press on the bottom with the force necessary to drive the given stone into ice of a given density (usually porous); and with this maximum pressure to move at the maximum rate of a third of an inch in a quarter of an hour! Try to saw a piece of marble through (with edge of iron, not of soppy ice, for saw, and with sharp flint sand for felspar slime), and move your saw at the rate of an inch in three-quarters of an hour, and see what lively and progressive work

you will make of it. I say ' a piece of marble,' but your permanent glacier bottom is rarely so soft " (*Deucalion*, i., pp. 261-263).

The older geologists were content to attribute to glaciers merely a transcendental power of surface erosion which enabled them to cut down valleys and to excavate lakes. Their more aggressive descendants go further, and actually argue that glaciers can break up their stony beds into angular fragments, then lift and pick out these angular stones and presently roll a large number of them into boulders. This extraordinary, but very fashionable, argument has been forced upon them by the difficulty of otherwise accounting for the fact that whatever moved the drift, did not merely bring a large number of stones from its far-off starting-place, but picked them up all the way along its route, when that route was a flat plain several hundred miles in extent, and when it was *ex hypothesi* buried deep underneath the ice mass.

The whole process is so contrary to every probability and to every experience that it would surely have been decent if those appealing to it had faced some of its elementary difficulties before propounding such a notion.

What weight would be required to break up solid beds of gneiss and other crystalline rocks *in situ* when the weight itself was moving so slowly as to be virtually quiescent ? This is surely a pertinent question. How, again, could we get such a pressure, or anything approaching to it, out of an ice mass which is much more yielding than any rocks it travels over, and which would itself crush into pulp and flow away long before this limit was reached ? There are some who have apparently faced this difficulty and found it an overwhelming one; who claim that the disintegration of the bottom beds was caused not by the weight of the ice-sheet, but by the expansion and contraction of these beds under very great disparities of temperature. Here, again, we have a postulate which is quite unverified and which seems at issue with experience. What evidence of any kind is there that there is, or can be, a variation of temperature at all (much less one capable of doing this work) underneath such vast ice masses as those postulated of the glacial age? Apart from this, let me quote from a keen, sharp-witted reasoner on such matters.

" It has been suggested," says Mr. Irving, " that the freezing of water within the crevasses and pores of the rocky bed of the glacier must, by its expansion, break up the rock surface, and thus furnish detritus for the glacier to carry away as the loosened materials are caught up by the ice. This, *prima facie*, seems a sound argument in favour of excavation. We must therefore examine it. We must recollect that (1) the water contained in this way within the rock is exposed to subterranean heat passing up by conductivity from below, and that if this is slow, owing to the low conductivity of the rock materials, the cooling effect of the ice of the glacier is, *a fortiori*, equally slow ; (2) the actual surface of the rock at any given point is either (*a*) in contact with the water of the glacier stream, which is not below 0° C., and therefore cannot freeze the water within the rock, or (*b*) in contact with the ice (or a stone stuck in the ice and at the same temperature as the ice), in which case the ice may be either at or below 0° C., since according to the pressure at the point of contact, as Helmholtz's reasoning shows, ice at 0° C. has no power to freeze water. Since with equality of temperature there can be no exchange of heat between the bodies—and if the ice be below 0° C. it can only be so at a pressure proportionately greater—this very pressure must be exerted upon the rock, and so counteract the expansive force of the water within the rock. The hypothesis is thus shown to be wholly inadmissible. Further, the appearance of glaciated rocks shows that they have not been thus broken up by freezing water while the glacier covered them " (*Journ. of the Geol. Soc.*, xxxix., p. 67).

The breaking up of the glacier's bed, however, is only one part of the problem. A more difficult thing even than this to explain is the process which is taken for granted by many modern geologists, by which a great mass of ice virtually stagnant, and still pressing down with the pressure of many tons to the square foot, is supposed at the same time to be able to drag up fragments of stone out of its own bed, like a carpenter takes nails out of an old box, or, as Mr. Bonney says, a dentist takes teeth out of an aching jaw. Thus Mr. Carvell Lewis, a great prophet of the new school, speaks of " a deposit composed of fragments torn by the glacier from the

basement rock over which it passes ". The process seems to me as reasonable as that the Nelson Monument should dig itself a coal-pit under its own base. The best proof that the theory is fantastic is that where we have very large and therefore very potent glaciers, there are no moraines where there are no corresponding *nunatakker* or projecting rock masses, proving that the moraines in Greenland are not derived from the glacier beds but from their backs.

If we turn from the consideration of what ice can do as an erosive or excavating agent, when tested by its physical qualities, to the actual results of its handiwork, where glaciers are now living objects and not merely dreams, we shall have the best of object lessons to guide us.

Viollet-le-Duc has a graphic description of a recently abandoned glacier bed. He says: "Rien n'est plus désolé que le lit d'un glacier fondu. Sable fin, blocs énormes, débris de toute taille et de toute forme, conservant leurs angles vifs, déposés non en raison des lois de la statique mais suivant le hasard, prêts à tomber sur leurs voisins au moindre choc; assiette striée, moutonnée, creusée dans les parties les plus tendres, présentant des mamelons aux points les plus durs, petits lacs tourbeuses dans les creux, aridité, couleur grise répandue sur toutes les roches, absence de végétation qui, indépendamment de l'altitude, ne saurait se prendre à ce sol dévasté et mobile, tel est le tableau que présente le lit abandonné par la glace." This graphic account is very easy to verify just now by those who will travel to the foot of the Rhône glacier, where I spent some time two summers ago, and which has retreated almost, if not quite, a mile during the last few years. Anyone who does so will see that this very large glacier has not dug into its bed at all, which is a flat plain covered with débris, through which runs the sprightly Rhône; and yet here, if anywhere, just after this mighty ice-river leaves its sloping bed and has the greatest momentum, it ought to have been excavating, if excavating were a function of glaciers. We should none of us be surprised to find that a glacier at the end of its nose were doing what a water-fall does very often, and by the mere weight and pressure and movement of its mass were scooping out a hollow there; but, so far as my experience goes, I know of no such case. I will

quote in support of myself some much more experienced travellers in mountain countries than I am.

"In 1818," says Charpentier, "the Glacier de la Tour, in the valley of Chamouni, advanced without ploughing up the ground about eighty feet over a gravelly bed not covered with earth, but at the end of that space it met with certain meadows whose soil being rather marshy ground was entirely upraised and overturned.

"In the same year the Bossons glacier advanced so that it was feared the road would be swept away, instead of which it produced no disturbance in the soil, which is there quite soft, arable ground.

"And now, the glacier of the Bossons having retreated immensely, what sort of a bed has it left? Bare ploughed rock? a scooped hollow? a carved ravine? Not at all; but a causeway of the loosest stones; an embankment which a railway would despise as far too loose to lay sleepers on, and this just where its slope and speed decreased—where the friction, according to the glacial theory, should have increased. As a matter of fact, whatever the glacier was expected to do, it has flowed over all the loosest soils, except downright mud, and except where, as at the Gorner glacier some thirty years ago, a soft bank stood right up in its way and was simply crushed but not eroded."

"In 1853, during a temporary extension of the Glacier des Bossons, it was stopped by a large rock in the way. The ice accumulated behind it until it acquired sufficient weight to carry it away" (Collingwood, *The Limestone Alps of Savoy*, pp. 170, 171).

Speaking of his experiences, Bonney says: "The centre Grindelwald glacier in the last stage of its descent passes over three or four rocky terraces. The angles of these are not very seriously worn away, nor are hollows excavated at the base of the steps. The bed of the Argentière glacier (I made my way some distance under the ice) was rather unequal and was less uniformly abraded than I had expected. There were no signs whatever of the glacier being able to break off or root up blocks of the subjacent schistose rock; it seemed simply to wear away promontories."

Referring to advancing glaciers, he speaks of what he had

seen them do. " They ploughed up the turf of a meadow for a foot or two in depth, they pushed moraine stuff in front of them, showing some tendency to override it, and nothing more. In the beds of the recently retired Glacier des Bois and of the Argentière glacier there was a stony plain. The glacier had not been able to plough up a boulder bed, even at a place where, owing to the change of level, some erosive action might not unreasonably have been expected. In both these cases big blocks of protogine were lying on the beds, striated on the sides and top, showing the ice had flowed over them like a stream of mud " (*Geol. Journ.*, 1893, i., p. 488).

, According to Whymper, the glaciers of Greenland leave uncovered in their retreat level surfaces without any sign of basins, and inequalities in the hardness of the rock masses produce little or no effect upon the surfaces worn by the ice (*Scrambles in the Alps*, p. 488).

Desor long ago pointed out that it had been clearly established that the Morteratsch glacier when it reaches level ground can and does move over the soft deposits in the bottom of the valley without disturbing them. Again, Dr. Wright points out how near the south-western corner of the Muir glacier in Alaska large trees in great numbers (some measuring ten feet in circumference about fifteen feet from their roots), which have been preserved for an indefinite period underneath the glacier, are now being uncovered, and appear standing upright with their branches intact upon them and their roots embedded in the soil in which they grew. A stratum of this soil even consists of moss and leaves and cones which originally formed a carpet over the forest floor. There can be no doubt, he adds, that after the accumulation of sand burying the forest the glacier advanced for a great distance over it, attaining a thickness at that point of two or three thousand feet.

Mr. I. C. Russell, speaking of the floor of the Malaspina glacier, says: " This is one of the many instances that might be cited where a glacier rests upon loose unconsolidated material which is not perceptibly disturbed by the imposed load ".

The Rev. A. Irving says the recession of glaciers within the last half century or two, leaving a plain strewn with rolled

débris, can scarcely be better illustrated than in the case of the Rosegg glacier. Here, as in many another Alpine example, the recession of the glacier shows no trace of excavation of the valley floor. In regard to the Muir glacier of Alaska Prof. Cushing of Cleveland, Ohio, says : " Those who hold the power of glaciers to vigorously erode hard rocks under most circumstances take, it seems to me, an indefensible position. At the Muir glacier, in just the position where the greatest erosion would naturally be expected, soft gravels have been undisturbed by the ice " (*Natural Science,* iii., p. 60).

" Last summer," says Ruskin, writing in 1879, " I was able to cross the dry bed of a glacier which I had seen flowing, 200 feet deep, over the same spot forty years before, and there I saw what before I had suspected, that modern glaciers like modern rivers were not cutting their beds deeper, but filling them up " (*Deucalion,* i., p. 423).

Von Haast speaks of a valley on the west coast of New Zealand " where the glaciers advanced over a deposit of such apparently incoherent nature as a gravel bed without destroying it to any appreciable extent " ; at another spot, he says, " the peculiar conditions of an assembly of beds go far to prove that glaciers when advancing again after their retreat do not always clear out their former channels of moraine deposits accumulated therein, and that even under favourable circumstances the finest gravel will, when protected by moraines of comparatively inconsiderable thickness, be so thoroughly protected that no change in its stratification can take place ".

Studer compares a glacier to a stream of lava, and says if the latter can dig out its bed, often composed of movable sand or tufa, as is sometimes urged, the appearance of volcanic cones, etc., would be very different. He also points out that where their depth is the greatest, glaciers are often underrun by streams which intervene between them and their beds and prevent their erosion, and goes on to say : " Sur le fond de gravier et de décombres d'une profondeur inconnue, qui s'étend en avant de nos grands glaciers à Chamouni, aux glaciers d'Arolla, de Ferpècle, de l'Aar et en avant de tous les autres, on ne voit pas la moindre trace de la prétendue tendance des glaciers à s'enfoncer en creusant le sol. Nous

savons aussi que, dans les régions où les glaciers atteignirent le bord de la mer, ils se prolongent au-dessus de l'eau et ne plongent pas sous son niveau " (Studer, *Bibl. Univ.*, 1864, p. 102).

In view of these facts and observations it is well to remember the tremendous dynamical work which has been attributed to ice as a denuder and excavator by some of its extreme champions.

Belt, in speaking of the so-called glacial period, says : " It is not simply a question of scratched blocks and transported boulders, the whole physical geography of the world has been affected by it . . . not only the valleys and fiords of the north, but the great plains of Europe and Asia were produced by it " (" Climate of the Glacial Period," *Quart. Journ. of Science*, xi., p. 463).

Tyndall urges that there are insuperable difficulties to the notion that the present mountains have arisen through the action of forces localised beneath their bases, or that the valleys as they now exist have sunk through want of local support underneath. A general elevation of the land must be assured, and the question then occurs how has the land thus elevated been carved into its present form, and he concludes that ice alone was competent to plough out the Alpine valleys. " That the glaciers were the real excavators seems to me," he says, " far more probable than the supposition that they merely filled valleys which had been previously formed by water denudation " (*Phil. Mag.*, 4th ser., xxiv., p. 171).

Tyndall's view was characteristic of his occasional extravagance. It was, when published, attacked not only by the geologists, but by the explorers of Alpine glaciers, and by none more sharply than by Ramsay and Jukes in this country, and Desor in Switzerland, all of them champions of extreme glacial views. I have already in my former work quoted the arguments of Ramsay, and will only here collect three or four additional opinions, and I will begin with that of my friend Prof. Bonney.

" Where glaciers have been," says Bonney, " toothed prominencies have been broken or rubbed away, the rough places have been made smooth, the rugged hill has been reduced to rounded slopes of rock, ' like the backs of plunging dolphins '. But the crag remains a crag, the buttress

a buttress, and the hill a hill; the valley also does not alter its leading outlines. All that the ice has done has been to act like a gigantic rasp. It has modified, not revolutionised; it has moulded, not regenerated. When we examine the ancient glaciers in the higher Alps we are struck by their apparent inefficiency as erosive agents. Where the ice has lingered longest, just beneath the actual glacier, we see that a cliff continues to exist. On the lee side of prominences crags still remain. . . . At Meiringen the valley of Hasli is barred by a craggy ridge which is cleft by the Aareschlucht, which the ice has moulded into billowy undulations. Why has the rocky rib remained when all the rest of the valley is so smooth and flat ?

"When we enter the upper part of the valley formed of crystalline rocks and extending up to the Grimsel, etc., what contours does the valley present? Everywhere, no doubt, ice-worn rocks, curving slopes extending far above the valley floor, spurs and ridges now one mass of *roches moutonnées*, but hardly ever the faintest approach to a trough-like section . . . yet the contours of ice action, and in some cases the very striæ, can be traced almost to the surface of the torrents." He quotes the Val Bregaglia, the Val Mastallone, the Val Anzasca, the valley of the Drance, etc., as examples of a similar kind, in which the minor features only are due to ice action, which has rasped and rubbed and left the rock faces worn and defaced, but still there.

"Even on existing data," says Ruskin, "the idea of the excavation of valleys by ice has become one of quite ludicrous untenableness. At this moment the principal glacier of Chamouni pours itself down a slope of twenty degrees or more over a rock 2,000 feet in vertical height; and just at the bottom of the ice cataract, where a water cataract of equal force would have excavated an almost fathomless pool, the ice simply accumulates a heap of stones, on the top of which it rests " (*Deucalion*, i., p. 257).

The same writer again says very truly: "If the glaciers of Chamouni were cutting their beds deeper, either the annual line of débris of the Mont Blanc range on the north side must be annually carried down past the Pont Pélissier, or the valley of Chamouni must be in process of filling up, while the ravines

at its sides are being cut down deeper. Will any geologist supporting the modern glacial theories venture to send me his idea on this latter—by him inevitable—hypothesis of the profile of the bottom of the Glacier des Bossons a thousand years ago and a thousand years hence?" (*Deucalion*, i., p. 72.)

Freshfield says: " Surely, if the Alpine valleys had been eroded by ice, those which date from a very early stage of valley formation, and have carried off the drainage of the largest snowy basins, those, that is, which have for ages contained the most powerful glaciers, should be the deepest. Take a familiar region—the Bernese Oberland. We find that the contrary is the case; the Aletsch glacier lies in a trough higher than that of the Viesch glacier; the Grindelwald valley is shallow compared to that of Lauterbrunnen. Or take mountain groups. Why was the Ober Engadine left at so insignificant a depression?" (! ! !) Addressing the supporters of glacial erosion, he says: " How can you expect it to be believed that a tool which made the enormous excavations of the lake of Geneva, or of the valley of the Rhône, broke down before such an impediment as the crags of Sion? Are you honestly satisfied with the answer that they owe their survival to exceptional toughness? Have you any adequate proof of such toughness at Sion or at Arco or at Bellinzona " (*Proceedings of the Geol. Soc.*, 1888, p. 786).

Bonney says again: "The ice has occupied these valleys, but has not materially deepened, excavated or modified the glens. Crags, as it advanced, must have risen up like peel towers from the floor of the valley, have been buried deep below the frozen mass, and have emerged worn, rounded, scored, but only so far changed as to become humps " (*Geol. Journ.*, 1893, pp. 486, 487).

J. W. Spencer, who has done so much to illustrate the origin and history of the American lakes, says of the gigantic valley in which they lie and the supposed glacial traces which it shows : " The striæ are nowhere parallel to the direction of the escarpments, whether these be submerged or above the level of the lakes, where they form bold topographical features; nor on the vertical walls of the limestone escarpments polished by lateral glaciation; in short, the striæ are at considerable angles, even at right angles to the rocky escarpments. Thus

it appears," he adds, "that the valleys were not shaped by glacial action" (*American Geologist*, xiv., p. 292). Penck, the most aggressive and extravagant of continental ice prophets, is at one with Desor and others in regard to the slight effect which glaciers have had in eroding valleys (*Die Vergletscherung der Deutschen Alpen*, chap. xxviii.).

Desor brushed away the notion of the valleys having been excavated by the glaciers in a contemptuous sentence or two. Speaking of Tyndall's theory, he says : " Nach diesem Physiker, welcher sich in anderen Gebieten durch ernste und wichtige Arbeiten einen wohlverdienten Ruf erworben hat, hätten die Gletscher sich nicht darauf beschränkt, Seebecken anzuhölen. Alle Alpenthäler seien vielmehr ihr Werk, so dass vor der Ausbreitung des alten Eises die Alpen nur eine gleichförmige Erhebung ohne Clusen, Comben oder sonstige Thal schlachten irgend einer Art dargestellt hätten. *Wir halten es fur ueberflüssig eine solche Theorie zu widerlegen. So hoch wir auch die Gewalt der Gletscher anschlagen, so glauben wir doch nicht, dass man im Ernst daran denken kann, ihnen solche Kraftäusserungen zuzumuthen*" (E. Desor, *Der Gebirgsbau der Alpen*, pp. 117, 118).

To this I have nothing to add. It condenses very well the objections to Tyndall's theory contained in my former work (*Glacial Nightmare*, pp. 608-615), and I doubt if anyone now maintains that theory.

While the notion that valleys generally were excavated by glaciers is probably extinct, there still remain a considerable number of devotees of the view originated by Dana and pressed by many ultra-glacialists, that some special kinds of valleys, like the fiords of Norway, Greenland and North America, were in some way the handiwork of the glaciers. This notion I have also analysed at length in my former work (*Glacial Nightmare*, pp. 622-628). I have shown how preposterous it is, and contrary to every form of induction, to suppose that these cracks and fissures with their straight up sides and their divergent subsidiary fiords should have been cut down by ice, and I have supported my own view by those of explorers of the first rank. The notion seems to have originated in the fact that for the most part fiords occur in high latitudes and not in low ones. This is to some extent so, but only because in the tropics and the lower latitudes the

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coast districts are almost entirely sandy and flat or composed of quite unsuitable rocks for such phenomena. But it is a mistake to suppose that fiords are confined to glaciated districts; and perhaps the most important of them occur in those parts of the higher latitudes where, by the concurrence of all observers, the evidences of a so-called glacial period are quite wanting—namely, Alaska and British Columbia. But this is not all. The old fiords of temperate regions exist abundantly enough, but are disguised and choked by gravels and soft deposits, as in Dorsetshire. Fiords occur also, as Falsan and others have pointed out, in Asia Minor, in Dalmatia, in the Asturias, in Spain and Brittany, in the desiccated bay of Carentan in Normandy, and in the Republic of Granada between the Atlantic and the lake of Venezuela (see for other examples *Glacial Nightmare*, p. 627). But they occur actually in the tropics, as in the Andaman and Nicobar Islands, and in Central America. They also exist in another form, namely, as buried valleys. The submerged valley of the Congo, running out into the Atlantic at great length, is merely a submerged fiord; so is the Golden Gate at San Francisco. It is, therefore, a more than usually preposterous argument when fiords are attributed to the handiwork of glaciers, not because the efficiency of the latter to cut them out has been proved, but because they are supposed to exist only in glaciated districts. I will quote an additional example, which is particularly interesting.

" On the sides of this chain " (the Urals), says Murchison, " where no glaciers have ever so acted as to have produced erosion, we meet with both longitudinal and transverse deep fissures in some of which lakes and in others rivers occur. Thus all along the eastern flank of the Ural Mountains we find a succession of depressions filled with water without a trace on the sides of the bare and hard rocks which subtend these lakes of any former action of glaciers " (*Journ. of the Geol. Soc.*, xxxiv., clxxii.).

These facts entirely destroy the basis of Dana's argument in favour of the excavation of the fiords by the glaciers, *viz.*, because they were supposed to be found only in so-called glaciated districts. This has been, so far as I know, the only argument forthcoming from the champions of that view.

They have never faced the stupendous difficulty of explaining how glaciers any more than water could cut down clefts in solid crystalline rocks with perfectly clean sides to depths of thousands of feet where the erosive power of ice is so small, as it has been proved to be by very many observations. Nor have they produced a rational explanation how to account for the peculiar conformation of the floors of the Norwegian fiords—which, after gradually sloping down to depths of several hundred feet below the sea level, rise again with a more or less sharp turn upwards at their throats, and this, too, in an area where the ice was so potent, according to the glacial champions, as to have been able, after traversing the fiords, to travel over the bottom of the whole breadth of the North Sea in spite of its broken contour. Ice, be it remarked, so far as we know its physical nature, must, when it gets embayed in a hollow, rest there as peaceably as death, while any motion left in it must be confined to its upper layers, which, in such a case, would flow over the embayed part. This is independent of the fact that even on a flat surface the only motion we can conceive ice making would be the overflow of its upper beds over the lower ones.

I will not press these arguments further, but will merely call attention to the immense depth and size of many of these fiords as another difficulty to be met, for all the work of excavating them according to the glacial champions must have been done in glacial and post-glacial times. The Hardanger fiord is about 400, the Sogne fiord 600, the Nordfiord 300 fathoms deep. The heads of the innermost branches are nowhere at a greater distance than some twenty miles from the watershed of the country, and the necks between them nowhere as much as forty miles across (*Nature*, xlix., p. 364). This extent of the fiords is especially noticeable in the submarine ones.

Dr. Wright refers to the gorge, or more properly fiord, of the Saguenay, which joins the St. Lawrence below Quebec. " The great depth of this fiord," he says, " is certainly surprising, since, according to Sir William Dawson, its bottom for fifty miles above the St. Lawrence is 840 feet below the sea level, while the bordering cliffs are in some places 1,500 feet above the water. The average width is something over a mile " (*Man in the Glacial Period*, p. 197).

Spencer speaks of these drowned fiords as deep valleys often of great length, extending from the mouths of the existing rivers and crossing the American coastal plains over deeply buried channels. They are plainly recognisable in soundings upon the submarine coastal plateaux and along the banks and islands of the neighbouring West Indian seas to depths of 12,000 feet or more before reaching the ocean floors. They are often recognisable for hundreds of miles in descending to the floors of the ocean basins, as may be seen among the Bahamas. Lindenkohl traces the Hudson river channel to a depth of 2,832 feet and the Great Egg Harbour channel to 2,334 feet, where the plateau is submerged only 600 feet. The Delaware and Susquehanna valleys are also recognisable on the sub-coastal plain to depths of about 3,000 feet. Spencer showed in 1889 how the Laurentian valley was submerged for a distance of 800 miles beneath the waters of the Gulf of St. Lawrence, with a channel from 1,200 to 1,800 feet below the surface of the sea, but near the edge of the drowned plateau it descends abruptly to a depth of 3,666 feet. The same is true of the valleys crossing New England, Nova Scotia and the Newfoundland banks. From the edge of the continental shelf, the Susquehanna valley descends precipitously to a depth of more than 9,000 feet, with its valley recognisable to 12,000 feet. The Delaware descends abruptly to 6,066 feet, and it is plainly traceable to 11,256 feet, and to greater depths beyond. From the borders of Massachusetts, Nova Scotia and the Newfoundland banks the valleys descend precipitously into amphitheatres 6,000 or 7,000 feet below the surface, and continue to depths of 12,000 feet, and in some cases to even 15,000 feet (*Geol. Mag.*, 1898, pp. 35, 36).

Mr. J. W. Buchanan found the Congo submerged channel or fiord to extend eighty miles into the ocean and to a depth of more than 6,000 feet. Thirty-five miles off the coast the width of the submerged channel or cañon was forty-six miles, with a depth of about 3,450 feet, its bottom being more than 3,000 feet below the sea level on either side, while at a distance of fifty miles from the coast it reaches a depth of 6,000 feet below the sea level.

Buchanan describes another deep submarine valley or fiord

2,700 feet deep, situated on the African coast, 350 miles north of the equator, and he says that a similar valley exists in the south of the Bay of Biscay (*Amer. Journ. of Science*, xlvi., pp. 116, 117).

I need not add anything more in regard to the glacial erosion of fiords, which, as I have said, I analysed at great length in my former work. I also then examined the notion of the glacial origin of cirques and cooms (*op. cit.*, pp. 615-622). I confess that of all the surface features of the earth which have been attributed to ice these amphitheatres of rock with perpendicular faces, and often having lakes nestling in them, seem the most difficult to explain by ice-work of any kind. Ice when it is resting in a hollow cannot erode, for it cannot move. If the ice fell into these hollows from above, and did the work by impact, I do not know whence it was to come, for there are no glaciers above the cooms or cirques. They are at the heads of valleys and not half way down ; most of them are high up, and, as has been remarked, at this height the glacier would be formed of névé and not of ice ; and Prof. Bonney, who has so ably demolished all other kinds of transcendental erosion by ice and still clings to the erosion of cirques, admits in his paper on ice as an excavator that névé would be a very poor eroder indeed. He seems to think, however, that in glacial times the amount of snow would be so much greater that the whole conditions would be altered. Here I cannot follow him. Granting that the snowfall would be greater, how would this affect the problem ? The conversion of névé into ice does not depend on the weight of snow alone, but on the melting of a large portion of the annual snowfall. It is not dry snow but wet snow we need in order to manufacture glacier ice, and surely the postulated glacial period would lower the snow line and not raise it, would bring the névé lower down and not lift the ice higher up. This has seemed to me a critical difficulty which has not been faced by the glacialists in regard to other supposed dynamical work by ice. I shall return to it presently, now I merely quote it as an additional and perhaps conclusive argument to put beside those contained in my former work and already referred to.

Let us now turn to the theory maintained so courageously

and, as I think, hopelessly by Ramsay and Wallace that lakes in many cases were the handiwork of ice. This implies that glaciers are not only eroders like ploughs and capable of planing down surfaces, but actually of digging holes *en route*.

In my *Glacial Nightmare* (pp. 629-654) I have entered at great length into the question, and have, I think, proved on every ground what is held by the great mass of geologists, namely, the ineptness of that theory. It is still maintained, however, by a few resolute controversialists, and I will add some arguments to those I previously used, and which I had overlooked, and some facts which have since turned up. As in the case of fiords, these lake hollows have been attributed to the action of ice on the ground that they mainly occur in glaciated districts. It is true they generally occur in mountain districts, which they necessarily must under any theory of their origin known to me, but that they only occur in glaciated districts is a preposterous notion.

Allen, in describing the surface features of Bahia, says : " Over this whole region (*i.e.*, the plateau of Bahia) there is an almost entire absence of loose materials on the surface . . . slight knolls and shallow basins alternate, which rarely differ more than twenty or thirty feet in elevation. In the rainy season many of these basins become filled with water, forming shallow lagoons, varying in area from less than one to more than fifty acres, from most of which the water evaporates in the dry season. . . . So numerous were these lagoons for more than fifty miles that it seemed natural to speak of this region in my notes as ' the Lake Plain '. Almost everywhere the elevations are evenly rounded, indicating that the rocky crust has been exposed to rain and probably long continued abrasion. But the absence of abraded materials seemed most remarkable ; very rarely were even loose boulders observed, although a few such were repeatedly noticed. At frequent intervals there were irregular holes in the rocks, usually nearly filled with water, to which the natives gave the name of caldeirãos. These caldeirãos are of frequent occurrence. Nearly all of the considerable number examined. proved to be genuine pot-holes, and some of them were of great size. The largest one I measured was elliptical in outline, eighteen feet long, nine or ten wide and twenty-seven

deep, with smoothly worn sides. These pot-holes often occur out on the plain, far away from any high land, and they are sometimes found excavated on the summits of slight bulgings on the plain or even on the top of a hill." This report by Allen was in fact incorporated by Hartt in his work on the geology of Brazil, and made a principal basis of his argument. Hartt remarked that both in the Organ Mountains and Bahia there are valleys without outlet, and in Alagoas many deep lakes in rock basins. I would ask, as I have asked before, whether if phenomena like these had been described from the Alps or from Nova Scotia they would not assuredly have been pointed to by extreme glacialists as the unerring footprints of great ice-sheets? It is clear that either these facts must be disputed or else the champions of ice-at-all-hazards must concede that rock basins and giant cauldrons can be made by other agencies than ice. If so, they can be made as well in one place as another.

Speaking of Lake Astangi in Abyssinia, which is a rock basin, Dr. Blanford, who is an extravagant champion of denudation, says : " It is very difficult to account for the formation of such a hollow as that of Astangi by any known process of denudation. . . . There is not the smallest trace of glacier action, and it may be fairly inferred that the denudation which would destroy all traces of glaciers would also fill up the lake with detritus " (*Geology and Zoology of Abyssinia,* p. 160).

Numerous basins also occur in the Nilghirries in Southern India, quite out of the reach of any glacial action.

The case of the Himalayas is an important one in this behalf. Mr. Latouche, who had explored the Himalayan glaciers, says that true rock basins are rare there, although the conditions for their formation, on Ramsay's hypothesis, are conspicuously present (*Nature,* xlix., p. 40).

Mr. Oldham, of the Indian Survey, contests this statement, and says : " After a tolerably extensive experience of the Himalayas, I should be inclined to say that rock basins are of fairly frequent occurrence of all sizes, from the largest to the smallest, *but they are almost without exception filled with stream deposits . . . they are usually found where there are no traces of glacial action to be seen, and at levels to which we have no reason to suppose that glaciers ever reached."*

Elsewhere Oldham refers to the existence of lake basins in Beluchistan, which, as he urges, is a district entirely outside the possibility of any glacial action in recent geological times.

This shows how futile is the notion of those who associate the lake basins and glaciation as cause and effect because of their being sometimes found in the same areas. On the other hand, it is a remarkable fact that in one at least of the great mountain chains of the world characterised by enormous glaciers, lake basins are virtually absent.

The deficiencies as well as the excellencies of the Caucasus must be noted, says Freshfield. " It possesses no remarkable waterfall, no lakes and few tarns, neither sub-mountainous lakes like Como, Garda, Geneva, Lucerne, nor clusters of tarns like those that dot certain crystalline districts of the Alps. . . . The absence of lakes must be faced by those who believe that the great prehistoric glaciers excavated lake basins. . . . I disbelieve, " says Freshfield," for reasons I have set out elsewhere, in the excavation by moving ice of rock basins " (*Caucasus*, ii., pp. 52, 53). This is a sufficient answer to those who attribute the manufacture of rock basins to glacial causes, because such basins are supposed to be characteristic of glaciated districts.

Ramsay did not content himself with this argument (which we have shown to be untenable), however. He tried to argue the case on physical grounds. He laid it down that when such a body as glacier ice descends a slope the direct vertical pressure of the ice will be proportional to its thickness and weight and the angle of the slope over which it flows. If the angle be 5° the weight and erosive force of a given thickness of ice will be so much, if 10° so much less, if 20° less still, till at length, if we imagine the fall to be over a vertical fall of rock, the pressure against the wall (except accidentally) will be nil. But when the same vast body of ice has reached the plain, then motion and erosion would cease were it not for pressure from behind.

To this statement Mr. Carrick Moore replied : " The professor by ' the direct vertical pressure of the ice ' means that resolved portion of the weight which is at right angles to the slope, and this resolved portion, which is stated rather loosely to be proportional to the angle, is proportional to the cosine

of the angle, a function which up to 90° diminishes as the angle increases. It does not appear to have struck Prof. Ramsay as strange that by his theorem the erosive force is nothing at 90°, comes into operation as the angle declines from 90°, goes on increasing *sine limite* as the angle diminishes, and just when we expect it to be a maximum we are told it is nil as the angle vanishes." Mr. Moore then goes on to complain that Ramsay has mistaken weight for erosive force; but mere weight does not erode; weight in motion will. The erosive force of a body sliding down a slope is compounded of the pressure perpendicular to the slope and the velocity. According to Hopkins, the velocity of sliding ice is nearly uniform, and may be taken as proportional to the force in the direction of the motion. Mr. Moore translates this into the simple formula that the erosive force is as the pressure vertical to the plane multiplied by sin θ, θ being the inclination, that is, as weight × cos θ × sin θ, that is, as weight × 2θ. " This expression," he says, " is in accordance with Prof. Ramsay's theory that when the angle is 90° the erosion is 0, and again, when the angle is 0 the erosion is nothing, but it is quite discordant from his view that the erosion is greater at an angle of 5° than at one of 10°, etc. In fact, the maximum will be at 45°, and this," he adds, " I believe to be in accordance with what takes place in rivers with highly-inclined beds.

" As soon as the glacier reaches the plain, erosion by sliding ceases, and if it moves it must be by propulsion, and if it excavates the materials ground down must be removed. It is difficult to conceive how this can have been effected but by running water, and that is contrary to the idea of a rock basin. . . . If there is to be no river, then how were, say, the last 100 feet of the depth of the lake of Geneva excavated? It is not *le premier pas qui coûte*, but *le dernier.* Even granting that the enormous mass which the problem supposes could be forced up a slope, what becomes of the fine fluid mud into which the rocky contents of the lake had been ground? The advancing face of the glacier cannot be presumed to have forced the water before it, for it is fissured in all directions ; and though a glacier is said sometimes to thrust pebbles before it, the watery mud would always subside into the depths."

Mr. Moore further remarks on the confession made by Prof. Ramsay that when the Rhône glacier met with the opposition of the Jura Mountains it would spread out in the direction of least resistance. " On the same principle," he says, " I should expect the Rhône glacier on issuing from the gorge to crawl along the plain, as glaciers are known to do, instead of seeking out resistance by burying itself 1,000 feet among hard rocks " (*Phil. Mag.*, vol. xxix., pp. 526, 527).

This acute criticism should be supplemented by that of the Rev. A. Irving, who says : " Those who hold that ice can excavate assume that it moves as a rigid mass. If it did so, its scooping power would be immense, but that it does not do so has been shown by Prof. Tyndall in his *Forms of Water* and elsewhere, and demonstrated experimentally by himself and Prof. Helmholtz of Berlin " (*Journ. of the Geol. Soc.*, xxxix., p. 62).

" Ice being viscous the greater part of its potential energy due to its weight is dissipated very largely and used up within the glacier ; and in regard to its excavating power the most important element is the differential motion of its upper part compared with its base. Measurements taken by Tyndall at the foot of the Tanil showed a movement of the portions near the surface more than double that of the base. *A fortiori*, this differential movement must be greater, owing to the greater retardation of the base of the glacier when on a horizontal bed—so much so, that the greater pressure acting at right angles to that bed (which varies, *ceteris paribus*, with the cosine of the angle of inclination of the bed) would seem to avail nothing, since the movement of the base of the glacier when on a horizontal bed would be *nil*. The only propelling force to which it could be subjected would be the shoving force acting against it from the weight of the glacier lying upon an inclined slope immediately above. Prof. Tyndall has shown us how this would act (*Forms of Water*, p. 180). When the glacier passes from a steeper to a less steep gradient the crevasses close up, the yielding property of the ice comes into play, the ice at the surface is thrown into a series of transverse terraces or huge wrinkles, and the differential motion is increased so much that stones of the median moraine which have fallen into the crevasses are brought again to

the surface. From all which it would appear that the movement of the *base* of the glacier upon a horizontal bed is *nil*; and therefore here, where a theory of excavation most requires it, its erosive action is almost *nil*. This reasoning seems further confirmed by observations made on the Morteratsch. Some distance up the glacier the movement at its maximum was found to be fourteen · inches *per diem*; yet at the snout, which lies on a nearly horizontal bed, even without any ice in front to offer any resistance to its motion, the motion forward was only two inches in a day. It is no reply to this argument to say that higher up the erosive power must be greater. The ordinary law of valley contour, the steepness of the valley as a rule increasing as we approach its head, is well known ; and it follows from this that the biting power of the glacier upon the rocks (which diminishes with the steepness in proportion to the cosine of the angle of inclination) is less as we ascend into the steeper slopes of the glacier region. Moreover, it is self-evident that it is not on such steeper parts of the valley that the advocates of the erosion theory would call its action into requisition" (*Quart. Journ. Geol. Soc.*, xxxix., pp. 66, 67, 96, 97).

Ramsay further argues that the reason why lake basins are not found at the bottom of the glacier slopes, but some distance lower, is due to the piling up of the ice there in consequence of the moving layers crowding on each other; but it has been pointed out that in most Alpine and other valleys this could not happen, since they spread out at their embouchures, so that the ice would also spread out fan-like and would not pile up.

Turning from these *a priori* arguments to the results of observation, I will first quote Mr. Freshfield. Writing in the *Proceedings of the Geographical Society* in 1888 he says that for twenty-eight years he had had constant opportunities of observing glaciers at work in the Alps and elsewhere, during which time there had been a general retreat of the ice following on an advance, and he continues : " Let us look at the former beds of some of the thickest and swiftest glaciers of the Alps—the Brenva, the Bossons, the Lower Grindelwald. What have they left behind them in the valleys they occupied thirty years ago? Not hollows, but hummocks. Examine

the glaciers Shkara and Adish in Svanetia. The former has in recent times retired quite a mile. The extreme terminal moraine has been washed away, but four or five huge blocks, lying in a curve, mark its former position, and behind them the old bed of the ice is an uneven wilderness of rubbish heaps and shallow pools formed by the damming up of springs. . . . I need hardly multiply Alpine examples, which so many readers can supply for themselves. Burnishing, I have found glaciers often, scraping or pushing back soft protuberances in their path, sometimes, but scooping or excavating, never. Nor, I fancy, have my friends Prof. Bonney, Mr. John Ball, Mr. E. Whymper, Mr. W. Matthews or Mr. Nicholls been more fortunate."

Again, the same writer says: " It appears physically possible, and even probable, that glaciers (especially glaciers the velocity of which at all approaches the measurements said to have been obtained of the motion of the Jacobshavn glacier in Greenland, forty-nine to seventy-three feet, or of another glacier on the western coast, ninety-nine feet per diem on the surface) might, when the angle of their bed changed from a steep slope to a level, exert a very perceptible scooping or burrowing force at the angle. I do not remember to have noticed any instance of such action among contemporary glaciers."

Freshfield, continuing says : " We may feel, I think, confident that we are carrying on our investigation fairly if we look for the most conspicuous results in places where not only must the mass of the ice have been greatest, but also its lower surface swiftest—in short, in the sort of places in which we should expect to find running water most efficient as an excavator.

" Now, it is quite clear that the lake basin generally selected as an example of glacial excavation by the advocates of this theory (Geneva) is not in such a position. The ice covering it must have been more sluggish and (owing to the well-proved tendency of glaciers to spread out fanwise when they have space to do so) less deep while it was excavating (I accept for the moment the glacial hypothesis) than in the gorge of St. Maurice, where no lake has been created. It would seem, therefore, that the bed of the lake of Geneva can hardly have been excavated by glacier ice. . . .

" It is clear that neither the glaciers of the present day nor those of the last glacial epoch can or could on the level delve *with their snouts* even into an ' incoherent shingle bed '. If they scooped anywhere it must have been where the body of the ice attained its greatest weight and velocity. Most of the great lake basins, however, are found under the snouts of the ancient glaciers, where the ice had neither its greatest weight nor its greatest velocity. There are, on the other hand, few important lake beds under steep slopes, where we might expect to find them. Some lakes are in positions which appear to exclude a glacial origin for their beds. Many glaciated chains have few or no lakes. Lakes and tarns are in many parts of the world distributed quite irrespectively of ancient glacial extension. There are no sub-Alpine lakes in the South-Western Alps, the Caucasus or the Pyrenees " (*Proceedings of the Geog. Soc.*, 1888, pp. 781-786).

" The conclusion that ice does not scoop out basins may be supported by other weighty arguments. When on the sides of a glacier the ice has impinged against a rock surface it leaves convex not concave surfaces; that is, it spends its force on rubbing down projections and does not scoop out hollows. Why should the under surface act differently ? Again, some Alpine lake basins—Lago de Lugano, for instance —do not lie in the central tracks of glaciers. In the Alps they are found in far the greatest numbers among the crystalline rocks of the Maritime Alps, Canton Ticino and the Adamello group. Again, if the presence of lake basins involves the former existence of glaciers, the former existence of glaciers ought surely to involve the presence of lake basins. The absence of lake basins in ranges once extensively glaciated must be, at least, a serious objection (hardly to be met by the hypothesis that they have been without exception silted up) to the theory of their origin we are discussing " (*ibid.*, p. 783, etc.).

Ruskin says : " When once the ice *with strength always dependent on pre-existent precipice* has cleared obstacles out of its way and made a bed to its liking there is an end to its manifest and effectively sculptural power. I do not believe the Glacier des Bois has done more against some of the granite surfaces beneath it for these four thousand years than the drifts of

desert sand have upon Sinai. Be that as it may, its power
of excavation on a level is proved to be zero. . . . As long as
the ice exists it has the same progressive energy, and indeed,
sometimes with the quite terminal nose of it, will plough a piece
of ground scientifically enough ; but it never digs a hole, the
stream always comes from under it full speed down hill.
Now, whatever the dimensions of a glacier, if it dug a big
hole, like the lake of Geneva, when it was big, it would dig a
little hole when it was little (not that this is *always* safe logic,
for a little stone will dig in a glacier and a large one build,
but it is safe within general limits), which it never does, nor
can, but subsides gladly into any hole prepared for it in a
quite placid manner, for all its fierce looks " (*Deucalion*, i.,
p. 266).

Sir W. M. Conway, a most experienced Arctic explorer,
speaking in 1893, said he had lately visited the largest
glaciers in the Karakorum range, and notably the Hispar,
which has retreated twenty-five miles and is still forty miles
long. He says it has not left a U-shaped but a V-shaped
valley behind it, and there is no trace of the valley having
been gouged out, and nowhere in the neighbourhood is there
any lake whatever. . . . I and many others have been under
glaciers, both in their upper, middle and lower courses, and
have never seen one excavating. They slip and slide in
the smoothest possible manner, and here and there do a little
scratching (*Geol. Journ.*, 1893, p. 503).

Viollet-le-Duc says : " In accord with M. Studer and the
majority of the Swiss geologists we cannot admit that glaciers
have eroded these lakes to the depths of three, four and five
hundred metres below the sea level ". After stating that he
had made a great number of observations in the country, he
continues in his own graphic phrases : " On peut admettre
que, si les glaciers ont une action latérale assez puissante pour
arracher des promontoires et élargir les vallées par voie de
limage, ils n'agissent que faiblement, relativement, sur leur
lit, que leur plasticité se prêtant à suivre les dénivellements
du sol, ils n'opèrent et ne sauraient opérer sur le lit comme le
ferait une gouge, se contentant de l'user, de le polir, de le
lubréfier, pourrait-on dire. Comment admettre que, à la
sortie de la vallée haute du Rhône, le glacier qui alors pouvait

s'étendre à l'aise, eut creusé le lit du lac de Genève jusqu'à une profondeur de 300 mètres au-dessous du plan d'eau actuel, tandis qu'il respectait en amont la digue de Saint Maurice, tandis que l'énorme glacier qui descendait du Mont Blanc avec une pression de 2,000 mètres, ménageait le Prarion et était obligé de se soulever sur le dos de cette digue schisteuse, bien qu'il fut resserré sur ce point ? Comment supposer que ces courants glaciaires, qui aujourd'hui se prêtent à toutes les irrégularités des soulèvements, et passent dans les rétrécissements naturels comme dans les filières pour se développer au-delà sur de larges surfaces, et n'opèrent dans les gorges que sous forme de brisure et d'érosion latérales, favorisées par la dislocation des roches exposées aux intempéries, eussent possédé une puissance de creusement aussi considérable sur un point particulier de leur parcours, alors qu'ils trouvaient de l'espace latéralement " (*Le Massif du Mont Blanc*, pp. 196, 197).

The views of Irving, Spencer, Bonney and others, in opposition to the theory of the glacial erosion of lakes, have in these latter years been sharply assailed by Dr. Wallace in some able and aggressive articles which appeared in the *Fortnightly Review*, in which he pressed for the most extravagant position maintained by Ramsay. These articles have appeared since my former work was written and demand notice. Wallace was answered by Bonney and others, and I will quote at considerable length from Prof. Bonney's admirable replies, which seem to me most conclusive.

They have been frequently urged by him, and I quoted them partially in my last work, but he has since condensed them more effectually in his volume entitled *Ice Work*, in which he especially considers Dr. Wallace's arguments on the other side in the *Fortnightly Review* for November and December, 1893.

Wallace in his papers professes to point out three criteria by which rock basins excavated, as he says, by ice are distinguished from ordinary valleys :—

1. They never present those peculiarities of contour which are not infrequent in mountain valleys, and never exhibit either submerged ravines or those jutting promontories which are so common a feature in hilly districts.

2. Alpine lake bottoms, whether large or small, frequently consist of two or more basins, a feature which could not occur in lakes due to submergence unless there were two or more points of flexure for each depression, a thing highly improbable even in the larger lakes and almost impossible in the smaller.

3. The contour lines in most river valleys run up the tributaries for a certain distance, so that on taking them at heights of "two or three or five hundred feet," these would "form a series of notches or loops of greater or less depth at every tributary stream with its entering valley or deeply cut ravine," while in the lakes of glaciated districts the water never makes inlets up the inflowing streams, but "all of them without exception form an even junction with the lake margin, just as they would do if entering a river".

To this Bonney replies that "no one of these statements is strictly accurate, and that some of the Alpine lakes not only exhibit a complicated form but also occur in situations where the excavatory force of the ice stream must have been very slight, while they are wanting in those where it ought to have been more potent. The lake of Lucerne and that of Lugano are curiously complicated in shape, and appear to occupy parts of the beds of valleys confluent from rather opposite quarters. Supposing, however, that the latter has been scooped out by glaciers, what explanation can be given of the Küssnacht arm, which runs out to the north-east at an angle of about 60° with the main body of the lake? Granted that the ice contingent from the direction of the Brünig Pass may have produced some deflection in the Reuss glacier (which would be the chief agent in the work of excavation, and must have taken the same path as the river), it hardly could have forced that glacier to send out an offshoot which actually bends back on its general course. In connection with this, another difficulty arises as to the origin of the lake of Zug, 650 feet in depth. Did the aforesaid offshoot descend from the top of the ' Hollow Lane,' and thus acquire, on a slope little more than 200 feet vertical, a sufficient plunging force; or was the work done by an arm extended from the old Reuss glacier, which first scooped out the lake of Lowerz? The former lake, which is deep in proportion

to its size, seems to be situated just at the place where the ice might be expected to be least active. Yet a third difficulty is presented by the lake of Lucerne. East of the part just mentioned it is almost divided into two by the mountainous promontory of the Bürgenstock. This must have seriously obstructed the path of the main ice stream, and ought to have either forced it aside to the west in the direction of Stanz, where it would have been encountered by the glacier from the Engelberg valley, or obliged it to struggle through a comparatively narrow gateway, on the portals of which an agent so potent should have produced a much more marked impression than is to be discovered.

" In the case of the lake of Lugano, it is urged that while the surrounding hill ranges are high enough to prevent the great ice streams of the main valleys from descending in any force into the area now occupied by the lake, they are not sufficiently lofty to originate an important glacier system.

" As to the inference that the comparatively regular outlines of the Alpine lakes indicate that they have been excavated by ice, opponents reply that Dr. Wallace has forgotten the fact that this effect is produced by the subsequent deposit of débris. Alluvial fans are formed only under certain circumstances at the junction of a tributary stream with the main river, while, in the case of a lake, the sudden arrest of the affluent water at once forms a delta and obliterates the irregularities of the shore. Draw your contour lines, they say, slightly higher up the hillside, often but a few yards above the present level of the lakes, and along ground over which the ice stream must have passed, and the same class of outlines will be found in these as in all other valleys. Further, while the shapes of the lakes are admitted, partly for the reasons just given, to be generally somewhat regular, the rule has many exceptions, as, for instance, the great fork in Como, the headland of Sermione in Garda, the irregular shapes of the Lac d'Aiguebelette and the Lac de Breney, the islands in Maggiore and Iseo; and in Annecy we find not only the headland and island at Duingt, but also the vast ' pothole ' at Bourbioz and the submerged steep-sided hill near the Crêt-de-Châtillon. Sometimes the actual channel of a river can be traced for a certain distance along the beds of the

lakes and their contours under water are very closely related to those of the mountain side above, while a ' single sheet of M. Delebecque's atlas shows what varied forms these lakes can assume ; and the work as a whole presents to us a number of basins, some lying in the path of the great ice streams, others quite out of it, others again in regions which only can have been invaded very incompletely or for a short time by a glacier, and of these certain lie transverse to its path and parallel with protecting ridges '.

" The difficulty of the two or more basins which are often noticed in the bed of a lake is thus answered. Commonly these variations in the depth only amount to a few yards vertical, and are very probably due, as M. Delebecque has pointed out, to the irregular deposit of morainic material on the present lake-bed, while in the case of the larger lakes the occurrence of a slight undulation in a general line of flexure would not be surprising " (*Ice Work*, pp. 80-87).

Again, Mr. Bonney says : " Of the two important valleys in the Alps, one has a lake and the other has not. In one mountain chain lakes are comparatively frequent, in another very rare, if not altogether wanting, as, for instance, in the Himalayas, the Caucasus and the Pyrenees. In the last-named chain the glaciers, though not equal to those of the Alps, were far from insignificant—greater, at any rate, than the ice streams of the Jura—which, whether they were of local origin or straggling offshoots from the main Alpine masses, must have excavated lake basins if the hypothesis under discussion be correct. The Pyrenean glaciers were occasionally between forty and fifty miles long. They came down to the lowlands in situations very favourable to a process of digging ; yet there are no lakes. Again, Nicaragua and Titicaca, and· the lakes in San Domingo and Porto Rico, in Celebes and Tasmania, bear much resemblance to some of the lakes of the Alpine region ; yet these cannot be attributed to the action of ice, for Agassiz's dream of a vast glacier in the valley of the Amazon has been long dispelled ; and though Tasmania, as will be seen, once had its glaciers, they were mostly, if not wholly, in a different part of the island from the lakes " (*ibid.*, p. 88).

" Some of the big lakes," says Bonney elsewhere, " like Constance, Geneva, Como, Maggiore, etc., are comparatively near

the lower limits of the great ice-sheets, and so would be covered but for a short time. All of them are many miles from the ends of the existing glaciers, yet we are asked to admit that a rock basin, in depth sometimes exceeding 1,000 feet and generally more than 500 feet, has been scooped out in a time much shorter than that which has proved insufficient for the obliteration of the original features of the upper valleys or for the deepening of their beds by a few yards." Bonney asks what there is in the physiognomy of these lakes to quicken the glaciers from an inert to an energetic condition. There is no marked change of the level of the ground, no remarkable confluence of valleys, no conspicuous straits through which the eroded ice streams were forced by the relentless pressure of the masses behind. Surely Como cannot be accounted for by the slight descent from Chiavenna, or Geneva by that from the rocky barrier of St. Maurice, or Brienz by that from the Aareschlucht, while Constance, Zürich and Wallenstadt, Maggiore, Orta and Garda are hopeless puzzles. Moreover, what are we to say of the Achen See, that deep lake so strangely nestling among comparatively low limestone peaks; or of Zug, half sheltered by the block of the Rigi; or of Lugano, with its radiating arms enclosed on almost every side by mountains comparatively low. Speaking of the lake of Garda, which is in the path of a glacier, Bonney says : " The crags and headlands in the middle of the lake are curiously unlike, in their general outlines, what might be expected as the ruins left in the track of a gigantic scoop which has dug out a basin in one place full 900 feet deep ". Speaking, again, of the Küssnacht arm of the lake of Lucerne, he asks how it was produced. " Did a glacier plunge headlong down the little slope made famous by the legend of the ' hollow way,' or did the ice stream either from the Brünig Pass or from the Engelberg Thal crawl across the back of the glacier of the Reuss Thal, like one snake over another, and then compensate itself for this feat by excavation ? Perhaps such an intertwining of ice streams would not be too great a trial for the faith of some glacialists, but, speaking for myself, I should like to be supplied with a few corroborative facts before removing it from the imaginative poetry to the sober prose of science " (*Geog. Journ.*, 1893, p. 491).

Prof. Spencer, who has devoted many years to the investigation of the great American lakes, has shown, it seems to me most clearly, that they are not lakes of erosion at all, as it was thought by the older writers, but due to warping and deformation of the surface, accompanied by the filling up of old valleys with drift. I quoted at considerable length from his papers in my former work. Since then he has continued his investigations, and notably in a paper on the origin of Lake Erie, published in 1894, he has applied the same criterions to its explanation which he had previously done so successfully to Lake Ontario and Lake Huron (*Amer. Journ. of Science*, xlvii., p. 207, etc.), and has also reviewed the whole history of the origin of the lakes in the *American Geologist*, vol. xiv. I may add that when he brought his views before the Geological Society of London they were accepted by nearly every speaker there. I need not requote what I have already said in my former work in regard to Mr. Spencer's views.

In my former work I also devoted a considerable space to the analysis of a great many individual lakes once attributed to ice action, and since shown to have been inconsistent with such an origin. I will now add some others, not with the view of proving a universal negative by a number of individual examples, but to show that when tested anywhere the *a priori* and the empirical testimony to the incapacity of ice to do such work are amply supported by this kind of experience.

It must be understood that the issue is not a single one. First, is any lake in question a true rock basin, that is, is it enclosed all round with unbroken rock, like a tea-cup by its rim? or is it merely banked up by débris, clay, etc., and does it possess a disguised or open drain and efflux? Secondly, if it is enclosed in the fashion mentioned by continuous rock, has it been carved out by some method of erosion or is it the result of earth movements? To this latter part of the issue I shall devote more space in the next chapter, and will here merely mention some test examples of spurious rock basins.

Lakes, like ordinary reservoirs, which are dammed by clay dykes and barriers are of course out of the category of eroded lakes altogether, and yet it seems certain that they form the great bulk of lakes in mountain districts. The

Italian lakes offer notable examples of so-called moraine dammed lakes.

In the *Geological Survey of Pennsylvania*, 1884, Report ii., " Terminal Moraines," we read : " The occurrence of lakes is one of the most characteristic features of the glaciated area in the United States. A hundred thousand exist back of the terminal moraine, and almost none in front of it. They were at first ascribed to the eroding force of ice, under the supposition that they were scooped out in the solid rock, but facts suffice to indicate that their origin is due to filling up rather than to scooping out, to obstruction rather than to removal" (p. 29).

Let us now turn to a writer who has devoted much patient skill to the elucidation of this problem, namely, my friend Mr. Marr. In recent years a systematic examination has been made by him on a minute scale of some of the most typical districts where rock basins are supposed to have marked the excavating forces of ice, notably in Cumberland. He has specially examined the tarns, which more than the lakes have had a plausible ice origin attributed to them, and in virtually every case he has found that their waters are retained by dams concealing old channels.

" A casual glance," he says, " at many of these tarns shows that the stream which issues from them runs over solid rock close to the surface of the lake, and this appears to have been considered by some a satisfactory proof that the tarn itself occurs in a true rock basin." He then goes on to show that the great majority of tarns are in fact pieces of water held back by dams of moraine matter. " When the exit of the lake lies immediately over the old river bed the moraine matter is easily denuded. Accordingly we find numbers of instances of old tarns now converted into peat bogs owing to the temporary barrier of drift having been denuded, but in many cases, where the lowest point of the morainic barrier did not lie vertically above the bottom of the old moraine-filled valley, the stream would cut down rapidly until it reached the level of the rock, and then, in the majority of instances, the stream would cut side-ways along the junction between drift and solid rock until, when the stream reached its original position, the lake would be drained. But if a

subsidiary ridge of rock lay between the position attained by the stream issuing from the lake and the position of the former valley-bottom, denudation would be retarded to so great an extent (owing to the absence of sediment in the water where it issues from the tarn) that the lakelet would become much more permanent, and its depth would be the difference between the height above sea level of the bottom of the old moraine-filled valley and that of the present exit." Mr. Marr then goes on to argue that this being the case a mere examination of the exit of a lake gives no proof of the existence of a true rock basin. To show that such exists it is [necessary to prove convincingly that no former valley, now filled with drift, occurs which might have conveyed the waters away before it became blocked, and he adds that an examination of a large number of tarns has convinced him that a valley of this kind did exist in a large number of cases and may have existed in all the others which he had studied (*Journ. of the Geol. Soc.*, li., pp. 35, 36).

He then describes the examination of a number of these tarns, including (1) the tarns near Hawes Water, *i.e.*, Blea Water and Smaller Water ; (2) the tarns between Grasmere and Langdale valleys, *i.e.*, Easedale and Codale and Shekle tarns ; (3) Coniston tarns, *i.e.*, Levers Water, Low Water, Goat Water, Blind Tarn and Seathwaite Tarn ; (4) tarns between the Duddon valley and Wastdale, *i.e.*, Devon Water, Eel Tarn and Burn-moor Tarn ; (5) tarns in the Scawfell group, *i.e.*, Skyhead, Sprink-ling and Angle tarns ; and (6) tarns between Borrowdale and Thirlmere, *i.e.*, Watendlath Tarn, Blea Tarn and Harrop Tarn.

I do not propose to quote the details of the examination of these tarns, for which my readers must turn to the excellent paper of Mr. Marr, but will merely quote his conclusion, which is, that "in no case he had examined can the advocate of the rock basin say that the buried moraine valley does not exist. Until he can do so I feel justified in asserting that the tarns of Lakeland give no support to the theory that the basins in which they occur were hollowed out by ice. To prove this it must be clearly shown that a moraine-blocked valley *does not* exist, whereas in some cases I maintain that I have proved that it *does*, and in all the others which I have examined either that it probably or possibly does " (*ibid.*, pp. 46, 47).

In a second paper, specially devoted to a re-examination of Watendlath Tarn and to Hard Tarn, Helvellyn, and Hayes Water and Angle Tarn, Patterdale, Mr. Marr showed that in every case the lakelet was held in by a barrier of drift and was not in fact a rock basin. Dr. H. N. Mill, in his remarks on this paper, said that Prof. W. M. Davies of Harvard considered from the configuration of the larger lake basins in the same district that they also were produced in drift-blocked valleys (*ibid.*, lii., pp. 12-16).

In the *Geological Magazine* for 1898, Messrs. Marr and Adie discuss the lakes of Snowdon, and come to the same conclusion as Mr. Marr had done in the case of the Cumberland tarns, *viz.*, that they may all be found to have channels buried or blocked by drift, and therefore not be real rock basins. In regard to one of them, Cwmglas, they say : " This pool, like the upper one, is situated on a dip slope shelf between two escarpments, and the ice has merely rounded off the edges of the escarpments without altering their general character. It has acted like sand-paper, and there is no indication of such erosion as would produce a rock basin—quite the reverse. The same features may be noticed in the case of Sprinkling Tarn on Scawfell."

Again, speaking of Llyn Teyrn, east of the summit of Snowdon, they say : " Near this tarn and close to the path a glaciated rock shows intercrossing striæ, one set running east and west and the other about 30° east of north and 30° west of south. A better example of intercrossing is seen by the path bordering the shores of Llyn Llydaw, due west of the causeway. A *roche moutonnée* shows three sets of striations —one trending east and west, another south-west and north-east, and the third 35° west of north and east of south. These directions point respectively to the north of Snowdon, the Lliwedd cliffs and the cliffs immediately above the *roches moutonnées*, and were probably produced by glaciers coming from those directions at different times. We call attention to them to emphasise a difficulty which has often been felt if one assumes that glaciers can carve out rock basins and yet are unable to obliterate the striæ formed at other times " (*Geol. Mag.*, 1898, p. 58).

In a paper by Mr. W. A. Brend in the *Geological Magazine* for

1897 he analyses the surroundings of several lakes in Carnar-vonshire. Of these Llyn Ogwen, which is very shallow, he thinks is probably sustained by a drift dam. Of Llyn Ldwal, which Ramsay thought lay in a rock basin ground out by the old glacier, Mr. Brend says the soundings do not support this view, and he thinks drift damming alone has operated in the formation of the lake. Of Llyn Bochlwyd he says if it be a true rock basin it must be of considerable depth, as the drift at the exit is very thick. Moreover, it is difficult to account for the irregular outline of the tarn on the theory of glacial erosion. Close by this are Llyn Clŷd and Llyn Crôm. "Round neither of these could rock *in situ* be continuously traced." Fynnon-y-Lloer has about forty yards to the north a thick mass of moraine matter, and it is here, says Mr. Brend, that probably a dam exists, as the lake is apparently shallow. In regard to the three lakes, Llynian Duwannedd, Llyn-y-foed and Llyn Geirionydd, he was not able to ascertain whether they were rock basins or not; all have drift close by. In regard to the last he says the shape of the western margin makes any explanation of its formation by glacial erosion difficult to accept. The same uncertainty attaches to Llyn Cawlyd and Llyn Eigian. Mr. Brend concludes, from an examination of all the lakes he investigated, that in the case of Llyn Crafnant and Llyn Geirionydd, one almost certainly a rock basin, the other very possibly so, earth movements may have been wholly or partially responsible for their formation (*Geol. Mag.*, 1897, pp. 406, 407).

So much for dammed-up lakes.

Among what Desor called orographic lakes he puts lakes in synclinal valleys or lakes shaped like a boat, such as the lakes of Bourget, Joux and Saint Point, lakes in isoclinal valleys, like the lakes of Brienz and Wallenstadt, and lakes in transverse valleys which the French call *cluses*, such as the lakes of Thun and of Uri. Most of the Alpine lakes, accord-ing to Desor, are orographic.

Prof. Bonney, who has destroyed the reputation of many of the Alpine lake basins, allows that a few true rock basins exist, and names four in a recent paper (*Geol. Mag.*, 1898, p. 15, etc.), three of which are situated in cirques or very precipitous corries. One of these is the Lago Tremorgio,

situated 5,997 feet above sea level, on the south flank of the Val Bedretto; the second the Lago Ritom, in the Val Piora, opposite to and at almost the same height as the former. This lake is sixty metres in depth. Close by is a small lake called Lago Cadagno, which seems to be very doubtfully a rock basin, and is probably dammed by drift. The fourth is Lago Tom, close by.

These are the only four rock basins in the whole Alpine districts allowed by Prof. Bonney, and I am bound to confess that in every case it seems most unlikely, whatever their origin, that they should have originated in erosion when the available erosive weapons are so slight, as they must have always been, at the height of 6,000 or 7,000 feet. It seems almost certain that such lakes as these are due to slight earth movements. I shall have more to say of them in the next chapter.

Von Haast tells us that nearly every lake on both slopes of the southern Alps of New Zealand is surrounded at its lower end by a broad circumvallation of moraines (*Geology of Canterbury*, etc.).

Let us now turn to the individual lakes about which so many polemics have arisen, and to the more conspicuous lakes of the Alps and the mountain districts of England, which have been thought to be glacier dug.

Penck, who is a keen supporter of the glacial erosion of mountain lakes, is obliged to confess that the theory will not apply to them all. In regard to the Konigsee and the Zellersee, he says that the former has been already recognised as having been produced by the sinking of the surface, due probably to the solution and removal of beds of gypsum, and that the latter is a dammed up valley.

Mr. A. Irving points out that Heim, in his *Mechanismus der Gebirgsbildung*, has completely disposed of the case of the lake of Lucerne by the results obtained by a series of soundings carried out by himself and one of the engineers of the federal government, which show conclusively that the bed of the lake is *an alluvial plain* of an ancient valley which has been converted into its present state by a partial closure of the valley at the outflow of the Limmat. The deep Achensee, again, is completely explained as "a case of a fault-

CHAPTER IX.

SUBTERRANEAN FORCES AS FASHIONERS OF THE EARTH'S SURFACE.

> Diseasèd nature oftentimes breaks forth
> In strange eruptions ; oft the teeming earth
> Is with a kind of colic pinch'd and vex'd
> By the imprisoning of unruly wind
> Within her womb ; which, for enlargement striving,
> Shakes the old beldam earth and topples down
> Steeples and moss-grown towers.
> —Shakespeare, First Part of *Henry IV.*, Act. iii., Sc. i.

To a person who approaches the subject with a quite unbiassed mind, it must seem very strange that the great mass of geologists should in these latter days have so completely given up the faith of their fathers in substituting erosion for subterranean movement as the chief fashioner and sculptor of the earth's surface.

It is a good proof, perhaps, of the dominance of Hutton, Playfair and Lyell that the worship of Neptune should so largely have displaced that of Pluto among the teachers and professors of geology. It must be said that this has only been possible because men have shut their eyes to great classes of facts, and have determined, whatever the result, to have nothing to do, if possible, with what they call catastrophies. In some way or another they have got to think that any theory involving an appeal to forces more powerful and far-reaching than those in operation now is obsolete and unphilosophical, and that the real genuine philosopher is the man who in geology gets away as far as he can from the point of view of Sedgwick and Murchison and other founders of our science, whose reputation is very dear to some of us.

Some of us, indeed, hold that a great deal of this new departure is not science at all, and is bound presently to give

place to saner views, in which the teaching of the old masters will again prevail. In no part of our subject is this more true than in the question we are now discussing ; and I do not hesitate myself to confess, and to be proud of the confession, that I believe in the old men rather than in the new.

As we have seen in the last three chapters, the evidence points very strongly to the fact that rain, wind and frost, snow, river and glacier, have not been the shapers of the greater features of the world's surface. They have polished and smoothed, they have rounded and softened, they have covered ruinous and broken surfaces with soft mantles of clay and sand and gravel, but, so far as we know, they have not carved out mountains and valleys, fiords and lakes out of solid rocks, and if they had had eternity to help them, which they have not, they would, so far as we can judge, be incapable of doing work of this kind on this scale.

If this be so, and I do not see how the case I have tried to present can be answered, then it seems to me that Lyell's teaching, which has been grievously exaggerated by his scholars, has led those who have trusted it into the wilderness, and that we must go back and see if older explanations are not more rational.

If we look at the moon we shall have an object-lesson of great service in this behalf. The moon has no atmosphere, no rain, no snow, no rivers and no glaciers, and yet could any landscape be more torn and ragged than that of the moon, with its great craters, its jagged mountains, its valleys and rifts and fiords, its huge amphitheatres and cirques, its exposed cliffs and scarps, many on a scale unmatched on the earth? It is clear, then, that in this instance, at all events, it will not avail to make appeals to that erosion and denudation which are alone thought philosophical as mundane causes of earth sculpture by the new prophets. No one doubts, as far as I know, that the forces which shaped the moon's surface were subterranean forces, and that its face represents more or less in all probability the slag on the surface of a vast molten globe when it had cooled down and become quiescent. Quiescent enough, for, as is well known, a very minute examination for a long time has failed to show that any changes of any but the most shadowy kind are at present going on on the face of the moon.

If the earth was, as these same philosophers contend, once itself a molten globe which has gradually cooled down and become solid, it seems to follow, if we are to base our reasoning on analogy where we have no direct evidence, that there was once a time when possibly its surface was not unlike the moon's—torn and crumpled and ragged, like the surface of a lava current. And if the two are now different, it is not improbable that this is due to the fact that the earth has had an atmosphere, and that rain and wind and frost and river and glacier and sea have been at work in planing down and rubbing the asperities off that surface, and have been doing for the whole world what the plasterer and the painter do for the rough, raw walls of our houses when they are being built.

We have, on a small scale, in volcanic districts examples which show us what a mundane landscape must have been like originally. Among them, Iceland is a notable example. There the surface of the country is largely occupied by rough and broken lava which has cooled down, and presents in a small way a shadow of the moon's surface. There is, therefore, no *a priori* reason, but the reverse, against the notion that the surface of the earth was originally shaped by subterranean forces, and by the stresses and strains caused in great masses of tough rock when cooling, and therefore shrinking and contracting.

The shrinkage of the earth's cuticle, like that of a drying apple-skin, and the corrugations and surface contours thereby formed, are not, however, the only results of subterranean forces of which we have ample evidence. It is perfectly plain, if we visit volcanic districts where earthquakes prevail, that we have phenomena involving surface changes on the earth not to be attributed merely to the shrinking and consequent corrugation of its crust. The shrinking of the crust will squeeze the whole closer together and cause folds and twists and bends, but it will not usually, so far as I can see, of itself cause fissures and gaps and more or less deep hollows in the ground. No doubt, where the strata are much bent, they will sometimes crack on the concave sides of the curves, just like a rod of iron cracks when too much twisted and when not quite plastic enough, or as a homely piece of sealing-wax will when heated and similarly bent. And this, doubtless,

accounts for certain rents in mountain strata. But it is only part of another problem, namely, the application of more stress to a solid stratum than it will bear without giving way; and it is quite patent that in a great many cases this has not been due to the squeezing together of the strata, but to the application of some elevatory or explosive force below the surface, which has stretched and not contracted them. It is only by a process of stretching and consequent dragging apart that we can explain some fissures and gaps in the rocks.

What I wish to emphasise is that, in addition to and apart from the corrugations on the rind of the earth-apple caused by its general shrinkage, there have been local disturbances involving explosive or elevatory forces acting in special districts and focussed about certain points which have left great stretches of horizontal beds close by undisturbed and untouched, and have upheaved and raised the surface contour in various ways, forces whose nature and potency are only dimly shadowed by those which have caused the phenomena of earthquakes and volcanoes during the human period. It is, nevertheless, in these shadows and ghosts of former catastrophes of which human records tell that we may find our best lessons in regard to the more important movements of former days.

Let us first turn to the classical island of Iceland, and see what it has to tell us about the formation of rifts and gaps and fiords, etc.

In Mackenzie's *Iceland* we read of the Almannagja as a deep and frightful fissure near Thingvalla, which has been formed with many others of smaller dimensions and another large one running parallel with it by the sinking of the ground during some of those terrible convulsions which have shaken Iceland in its foundations. " The whole rock bears the mark of having been affected by fire " (*op. cit.*, p. 206).

I will now quote from a more graphic writer, namely, Baring-Gould. He says : " A peculiar feature of Iceland is the *gja* (pronounced *gee-ow*). This is a fissure in the crust of the earth, formed by earthquakes or volcanic upheavals and sinkings of the land. These zig-zag rents run from north-east to south-west. The most remarkable are the Almanna and Hrafna gjas at Thingvalla, the huge chasm in Katla, the rift into which pours the Jökulsá at Dettifoss, and the Stapa,

Hauksvörthu and Hrafna gjas in Gullbryngasysla. The first mentioned extends for six miles, and is, in one spot, 130 feet deep. The Hrafna gja, or Raven rift, is somewhat longer, but only fifty feet deep.

" In 1728 there opened a chasm in the Öraefa of immeasurable depth. The Archdeacon Jon Thorlaksson, who visited it, found a large stone at one spot crossing the lip of the gulf. He and a companion dislodged it and sent it down into the abyss, but though they listened attentively they could not hear it reach the bottom. The great fissure of Katla has never been properly examined. It runs south-west to north-east, then turns at a right angle from south-east to north-west. . . . The only person who has been near the chasm is an Icelandic priest, Jon Austman, who ascended the mountain in 1823. He describes it as quite inaccessible, all progress being stopped by enormous walls of basalt and obsidian, *while other profound chasms radiate from the grand trunk or principal fissure* " (*Iceland*, Introduction, xxvii. and xxviii.).

Speaking of the Allmens rift, he says "it is a split in the lava extending nearly four miles to the root of Armanns fell. The river Oxera shoots over the north-west verge and flows for a quarter of a mile through the chasm, then breaks through a gap on the other side, rolls down to the plain, and pours into the lake close to Thingvalla church. The Raven rift bounds the place on the south-east. This is less remarkable than the other chasm, as the height of the walls is less considerable, though the length is somewhat greater. These chasms have been formed by the great plain between them having suddenly sunk, leaving sharp edges in lines at the sides, between which it has been depressed. The plain itself is full of fissures of great depth, half full of clear water. . . . The bed of the lake is full of similar rents. The greatest height of the Allmens rift is 130 feet. As the rent nears the mountains the walls are less lofty. The edge is splintered into chimneys, windows and mushrooms of rock " (*ibid.*, pp. 67, 68).

Describing a row of big blocks of lava, he tells us that on riding up to them and on examining the blocks he found that they overlay rifts, now all but choked up, miniature gjas, and that the force which had snapped the lava bed had tilted

the fragments of the broken edges, so that some blocks lay wedged in the crack, whilst others had dropped across it, and others again had fallen against each other. The lava thereabouts is very old and is covered with moss and grass, except along these lines. They run parallel to the direction of the flow of lava, and were formed by the edges of the molten stream cooling and resisting the tension of the still viscous centre (*ibid.*, pp. 193, 194). Huerfjall is a crater thrown up in 1748-1752. Along its base is a fissure in the lava similar to the Almannagja, only on a smaller scale (*ibid.*, p. 206).

Speaking of the famous Dettifoss, he tells us how the rock has been rent and a frightful fissure formed in the basalt about 200 feet deep, with the sides columnar and perpendicular. The gash terminates abruptly at an acute angle, and at this spot the great river rolls in (*ibid.*, p. 216).

It is currently believed in Iceland, and was stated in some of the public prints at the time, that a volcanic eruption or earthquake had taken place at Cape Reykjanes in October, 1887, by which a large new gja or chasm had been formed, separating a large rocky promontory, almost deserving the name of a mountain, from the main cape where the lighthouse stands. This chasm is at least fifty feet wide, and the captain of the steamer told Mr. Anderson he remembered the rocks before they were rent asunder (Anderson, *Proceedings of the Yorks. Geol. and Pol. Soc.*, 1892, p. 168).

Let us now turn elsewhere. Barnet, speaking of the western stream of lava at Teneriffe, says : " The ravines and rents are more formidable. . . . I can form no opinion why there should be these strange irregularities in the surface of this lava. In places it resembles what sailors term the trough of the sea, and I can compare it to nothing but as if the sea in a storm had by some force become on a sudden stationary, the waves retaining their swell. As we approached La Cueva there is a singular steep valley, the depth of which from its two walls cannot be less than 100 to 150 feet, the lava lying in broken ridges one upon the other, similar to the masses of granite rock that time and decay have rolled down from the Alps. . . . This current, like that of the eastward branch, has no resemblance to any lavas I have seen elsewhere ; it is

hardly at all decomposed" (*Geol. Trans.*, 2nd ser., v., p. 303).

Speaking of the great extinct volcanic crater at Aden, Mr. Burr says : " It has been cleft entirely through in one direction and partially so in another ; a great fissure ranges across it from north to south, and the two rents formed by it in the walls are called the northern and southern passes. The eastern half of the crater has evidently undergone a partial subsidence, as it does not rise to more than half the height of the western side, and it appears to have separated in two parallel lines at right angles to the great north and south fissure. . . . This great volcanic mass, although of commanding altitude and several miles in circumference, has evidently subsided like a cottage undermined by a neighbouring stream or river. . . . In subsiding the eastern side, which is an enormous mass, seems to have cracked again into three portions by fissures at right angles to the direction of the principal movement" (*Geol. Trans.*, 2nd ser., vi., pp. 500, 501).

The fissures formed in shrinking lava are not always fissures with parallel sides. Where the tension is greatest at the surface and is materially reduced as we go downwards they become V-shaped fissures, with their opposite walls gradually approaching each other as we go downwards.

A glacier, which, as we have seen, is a viscous mass, is yet very slightly viscous and in many ways behaves like a solid. When such a glacier is sliding down in a more or less gentle bed its surface remains intact and unbroken, as is known to all Alpine climbers. When it gets on broken ground, however, and especially when it has to pass over a hump in its bed, the strain upon it becomes so great that it first cracks and then gapes and forms what is known as a crevasse, which is merely a V-shaped cavity caused by the straining ice. What is familiar to every Alpine climber in the case of ice has been similarly noticed in the case of lava when subjected to a similar strain. It also gapes sometimes into V-shaped fissures.

These fissures, when the upward thrust is sensibly great, not only gape into V-shaped cavities, but the strata on each side of the cavity naturally tend to fall away from the cavity,

the thrust having tended to lift up the edges right and left so as to form a broken anticlinal.

In other and many cases, again, there has no doubt been a huge subsidence or sinking. Thus Bonney says : " Suppose that, for simplicity, any part of the upper portion of the earth's crust be represented by a sheet of ice covering the surface of a lake, and that from its bed either piles or pieces of masonry rise up here and there so as just to touch the bottom of the ice. If some of the water be drawn off, the ice above these obstacles remains immovable, but that in the interval sinks, cracking and bending as it slips down along their flanks, in the immediate vicinity of which the most marked disturbances will be produced. The piers represent ancient masses of solid crystalline rocks, the ice the less coherent sediments deposited on and about them " (Bonney, *Story of the Planet*, pp. 242, 243).

This process, or one very like it, has been repeatedly observed in the case of lava streams. In these cases the cooled and solid slag that covers the molten lava and is supported by it is sometimes forced to collapse, and to cave in by the draining away of the liquid from below it, the superimposed lava being too weak to sustain the pressure of the air, just as in Lancashire, when the coal is removed or the water which has taken its place is pumped out, the strata above cave in and form artificial meres and ponds and hollows.

An interesting and valuable paper on this subject was read before the British Association at Oxford in 1894 by Dr. Anderson. He supports the view that in many cases the valleys and hollows found in districts where lava occurs are due to the lower stratum of a molten lava stream having obtained an outlet after the surface has consolidated into a crust of greater or less thickness.

Gjas of this class, says our author, are confined within the limits of a single lava stream, and do not affect previously formed rocks. Usually there is a large gja roughly parallel with each side of the original lava stream, and a space between these has considerably subsided. Any gjas in this subsided portion are obviously of secondary importance. Examples of this are to be found in the well-known Almannagja at Thingvalla, which has a throw of about a hundred feet, while the

sides of the smaller gjas which enclose the Logberg in the subsided portion are practically on the same level.

There are several such subsidences near Lón and Asbergi in the north of Iceland. The main subsidence of Asbergi is a little more complicated, though evidently due to the same causes. Here a large roughly triangular area has subsided, the throw at the apex being probably nearly 300 feet, but a space in the middle has remained at its original height, so that a depression has been produced like a great V, the portions both between and outside the legs having remained standing. In the case of Thingvalla it appears not unlikely that the lava which flowed down into the lake solidified on coming into contact with the water, and formed a wall sufficiently strong to hold up the lava plain till it formed a firm crust, and that the giving way of this and the escape of the molten lower layers into the deeper parts of the lakes caused the subsidence. Similarly, the lava which escaped from Asbergi may have been that which now occupies the low ground near the estuary of the Jokulsá in the direction of Lón.

What applies to valleys applies also to the hollows in which lakes lie. Thus, speaking of lake Myvatu, Dr. Anderson says : " The barrier which holds up the water of the present lake consists of lava, and caves exist in it which are obviously channels by which molten lava has escaped. These and deeper-seated ones would be those by which the lava escaped, and left the depression occupied by the present lake. Between the craters of eruption and the lake there is a very remarkable series of rocks. The Dimmuborgir, masses of lava of fantastic shape thirty or forty feet high, have remained standing, while the intervening portions have subsided. They present slickenside marks where the subsiding portions have scratched the masses that have remained standing, and tide marks where the crust has halted in its descent; also in many places bulgings where the lava has been scarcely stiff enough to stand, and others where it has actually formed stalactitic masses." Dr. Anderson further draws attention to a subsidence on the slopes of Leirnukr, a volcano several miles north of this spot, where a large strip of land, perhaps 200 yards wide and one mile or more long, has been let down to a varying depth, averaging, perhaps, sixty to eighty feet.

The faults bounding it, like nearly all the fissures in this district, run north and south, and the east face, which is most perfect, cuts right through a thick stream of old columnar lava and through a large boss of tuff, round and over which the lava has bedded itself, and also through the tuff rocks at each side of the lava stream.

This great depression, which affects all the crust of the volcano impartially, Dr. Anderson suggests may have been caused by the falling in of one of the steam cavities, which may be presumed to exist under volcanoes after the lava has been expelled by the steam pressure, and he thus accounts for the observed fact that sedimentary rocks near volcanoes often dip towards those volcanoes (*Geol. Mag.*, 1894, pp. 516-518).

The phenomena I have been describing are not limited to volcanoes and outbursts of lava. Similar effects are sometimes caused by other means. Thus a curious phenomenon of an uncommon kind was brought before the Geological Society in 1889 by M. Corpi, namely, a great outflow of mud, amounting, according to his calculation, to 50,000,000 cubic metres, which took place in that year at Kantzorik in Armenia. The author describes how the great eastern mountain whence the outflow took place was crevassed in all directions, presenting for a width of 400 metres an enormous vacuity produced by the sinking in of a great part of its western flank, and showing a gigantic trench between this part and its base. This trench, this rupture of the mountain, the bottom of which could not be seen in consequence of a fold of the strata, served as the orifice for the escape of the mud, etc. The violent projection of the mud tore large masses from the mountain, and a large portion of the mountain has been carried away. A fissure and depressions of the ground were created at Nikhah, ten kilometres off, and crevasses of the same nature were produced two or three kilometres further on (*Quart. Journ. Geol. Soc.*, xlvi., p. 34). Similar effects have resulted from earthquakes.

I believe that a great mistake has been made in supposing that all earthquakes are due to one cause and testify to one phenomenon only. The earth quake or earth tremor, in which waves traverse solid strata sometimes for many scores of

miles and cause ruin to unstable buildings, etc., is only an effect and not a cause. Any jolt or shake or tear or upthrow or violent disturbance of the solid rocks anywhere will cause an earthquake. I suffer from a succession of small earthquakes from living too near a railway line. The explosion of a powder magazine causes a much greater and wider spread earthquake. The explosion of a powder barge at Erith, on 1st October, 1864, says Bonney, made the ground quiver even at Cambridge, and when 10,000 pounds' weight of gunpowder blew up in the mills at Mainz the tremor was felt more than a hundred miles away. More violent earthquakes still apparently have been caused by rents in the ground, due to various causes, which have not been always discriminated, and are not, in fact, quite capable always of discrimination. In many cases the fissures which cause the earthquakes are apparently very deep and sometimes wide and long, and they were doubtless much more extensive when the world was at times more active than it is now. Let us now turn to some earthquake phenomena as described by travellers, etc.

Dolomieu, describing the effects of the great earthquake in Calabria in 1783, mentions *inter alia* a fissure which opened and was several feet wide and two leagues in length, extending almost from St. George to St. Christina. Again he says : " Chasms and fissures traversed the flats and slopes in all directions, but generally parallel to the courses of the gorges in their neighbourhood. Between Polystena and Sinopolo these fissures were visible at every step." Again he says of the hill on which stands the town of Santa Christina, " a number of gapes and fissures intersect it from its summit to its base ". Speaking of the district of Terra Nuova, he says : " At the instant of the formation of one of the fissures and the separation of the mountain all the houses placed immediately above were perpendicularly precipitated down more than 300 feet and covered the bottom of the chasm with their ruins. . . . In all the environs on the edges of the valleys there has been considerable shrinkings. The whole plain above the town is intersected by numerous crevasses and fissures. . . . A part of the plantation of olive trees belonging to the Celestine monks sank some fathoms down, and all the re-

mainder is threatened with ruin from the number of fissures
and cracks which intersect it."

Again he says : " Of the fissures, several feet in width,
which extend from three to four miles, . . . in going from
Casal Nuovo to Santa Christina, within a space of six leagues,
one traverses a country intersected in a most extraordinary
manner by gorges, ravines and deep valleys; a country which
has consequently been the theatre of great revolutions. Not
a step can you take in this part without discerning either
fissures in the soil or places where the soil has fallen away."

Again he says : " The whole of the soil of the plain which
surrounds Casal Nuovo is sunk. This depression is particu-
larly apparent above the borough at the foot of the mountains.
All the sloping lands which leaned against this mountain
have slided lower down, leaving between the moving ground
and the solid, fissures several feet in width which extend
from three to four miles."

Again, speaking of the district of Terra Nuova, Dolomieu
says : " On the opposite side the mountain, by a perpendicu-
lar fissure from top to bottom, became divided, and one part,
separated from the other, fell in one block on its side, in the
same manner as a book opened in the middle which has one
part upright on its back while the other falls to the table."

Lyell describes at great length the phenomena of the same
Calabrian earthquake which occurred in a district where
plutonic rocks are not found. He tells us how at Cinque-
frondi innumerable fissures traversed the country in all direc-
tions. At Semmara a deep chasm was riven in a high platform
and the river immediately entered the fissure, leaving its
former bed completely dry. Lyell gives a figure of this
chasm, which is very like a coom or cirque. Near Soriana
innumerable fissures traversed the river plain in all directions.

In the earthquake at St. Domingo again, in 1770, innumer-
able fissures were caused in the ground. In the great earth-
quake at Chittagong, in 1762, *four hills are said to have been
variously rent asunder, leaving open chasms from thirty to sixty feet
wide.* In the Lisbon earthquake, in 1755, the mountains of
Arrabida, Estrella, Juleo, Marvan and Antra, being some of
the largest in Portugal, were impetuously shaken as it were
from their very foundations, and some of them opened at

their summits, which were split and rent in a wonderful manner, the rifts being nearly a mile in length and from 150 to more than 200 feet deep, usually straight, but some of them in the form of a crescent. Lyell figures one, which he says was by no means remarkable for its dimensions, which remained open by the side of a small pass over the hill of St. Angelo near Soriana. At Jerocarne the country was lacerated in a most extraordinary manner; the fissures ran in every direction, like cracks on a broken pane of glass.

At Polistena there appeared innumerable fissures in the earth, one of which, which is figured by Lyell, was, he says, of great length and depth, and in parts the level of the corresponding sides was greatly changed (Lyell, *Principles*, ii., chaps. xxviii.-xxx.).

" The convulsions which culminate in the formation of a volcano," says Geikie, " usually split open the terrestrial crust by a more or less nearly rectilinear fissure or a series of fissures. In the subsequent progress of the mountain, the ground at and around the focus of action is liable to be again and again rent open by other fissures. These tend to diverge from the focus, but around the rent where the rocks have been most exposed to concussion the fissures sometimes intersect each other in all directions. In the great eruption of Etna, in 1669, a series of six parallel fissures opened on the side of the mountain. One of these, with a width of two yards, ran for a distance of twelve miles in a somewhat winding course to within a mile of the top of the cone. Similar fissures, but on a smaller scale, have often been observed on Vesuvius, and they are recorded from many other volcanoes " (Geikie, *Text-book of Geology*, pp. 194, 195). Speaking of earthquakes, he says : " It has often been observed also that the soil is rent by fissures which vary in size from mere cracks, like those due to desiccation, up to deep and wide chasms. . . . Where the chasms are wide and deep enough to intercept rivulets, or to serve as channels for heavy rain torrents, they are sometimes further excavated, so as to become gradually enlarged into ravines and valleys, as has happened in the case of rents caused by the earthquake of 1811-1812 in the Mississippi valley. As a rule, each rent is only a few yards long. Sometimes it may extend for half a mile or even more.

In the earthquake which shook the south island of New Zealand in 1848, a fissure was formed averaging eighteen inches in width, and traceable for a distance of sixty miles parallel to the axis of the mountain chain. The subsequent earthquake of 1855 in the same region gave rise to a fracture which could be traced along the base of a line of cliff for a distance of about ninety miles. Mr. Oldham has described a remarkable series of fissurings which ran parallel with the river of Calhar, Eastern British India, *varying with it to every point of the compass and traceable for a hundred miles*" (*ibid.*, pp. 255, 256). This last sentence seems to imply that the river already marked an old line of weakness or perhaps of fissure. Lyell, in referring to the rents caused by the earthquake of 1848 in New Zealand, above referred to, says the rent was in a chain of mountains varying in height from 1,000 to 4,000 feet. He has other references to a similar effect.

In the Chilian earthquake of 1835 the earth, according to Captain Fitz-Roy, opened and closed rapidly in numerous places. The direction of the cracks was not uniform, though generally from south-east to north-west. In the earthquake of Bogota, on 16th November, 1627, wide crevasses appeared on the road of Guanacas. Other fissures opened near Costa on the plain of Bogota, *into which the river Tunza immediately began to flow* [the italics are mine]. In the Chilian earthquake of 1822, in a part of the coast formed of granite, parallel fissures were formed, some of which were traced for a mile and a half inland.

In the earthquake of New Madrid, Missouri, in 1811-1812, the earth rose in great undulations, and when these reached a certain fearful height the soil burst. Flint saw hundreds of these deep chasms remaining in an alluvial soil seven years after. The chasms usually ran from south-west to north-east.

Mrs. Graham, in her account of the earthquake in Chili in 1822-1823, says: " In all the small valleys the earth of the gardens was rent, and quantities of water and mud were forced up through the cracks. . . . The bed of the lake of Quintero was full of large cracks. . . . The granite on the peak is intersected by parallel veins from a line to an inch in thickness, mostly filled with a white slimy substance, but

some are only coated with it on their sides and present hollow fissures. After the earthquake of the 19th the whole rock was found rent by sharp recent clefts; very distinguishable from the older ones, but running in the same direction. Many of the larger of these clefts might be traced from the beach to the distance of one and a half miles across the neighbouring promontory, where in some instances the earth parted and left the stony part of the hill exposed " (*Geol. Trans.*, new ser., i., p. 414).

At Charleston the ground during an earthquake was fissured in places to the depth of many feet. Similar fissures were formed in South Carolina during the earthquake of 1811-1812 (Bonney, *Story of our Planet*, pp. 298, 299).

The fractures which accompany earthquakes are not limited to fissures. They include *inter alia* vast subsidences. These are due probably to the escape of gases or water, or other materials from subterranean cavities which have been tapped by earthquakes. " Some of the chasms which opened," says Lyell, " during the great earthquakes in Calabria imply the sinking down of the earth into subterranean cavities. One of these was observed by the academicians on the side of a hill near Oppido, into which part of a vineyard and a considerable number of olive trees with a large quantity of soil were precipitated. Yet a great gulf remained after the shock, in the form of an amphitheatre, 500 feet long and 200 feet deep " (Lyell, *Principles*, ii., p. 126).

Dolomieu describes the sides of the valleys as subsiding in terraces like an amphitheatre. Another interesting feature was the formation of lakes. Thus he says : " Les effets de ces éboulements ont été d'étrangler, ou de combler les vallées, par la rencontre et la réunion des bords opposés, de manière à obstruer le passage des eaux, et à *former un grand nombre de lacs* (il en a été formé dans ce moment plus de trois à quatre cents dans cette partie de la Calabre)."

Vivenzio says that during the convulsions in Calabria fifty lakes were formed. The Government surveyors enumerated two hundred and fifteen, including in them many small ponds. According to Grimaldi, many fissures and chasms, formed by the great shock of 5th February, were greatly widened, lengthened and deepened by the violent convulsions of 28th March.

Near the great rift in Iceland is a lake, ten miles long and from three to seven wide, which has been attributed to subterranean movements of the same kind.

Lastly, we have also in the observed phenomena of earthquakes cases of true faults, which are so common in the older rocks. Thus, Prof. Koto, in describing the great earthquake which took place in Japan in 1891, describes the formation of a visible fault line more than seventy miles long, with a relative horizontal displacement of its two sides along its direction of from three to six or even as much as twelve feet in length, and with a relative vertical displacement producing a step along the fault, whose height sometimes reached twenty feet (*Geol. Mag.*, 1894, p. 144).

This example from Japan is a singularly interesting one for the uniformitarian, and a good example of the danger of being too positive sometimes, for it really upsets a contention of a well-known living geologist, the Rev. A. Hill, in his arguments against rapid elevation of large masses of the earth. Not only so, but considering the number and size of the known faults, it is extraordinary that only in this quite subsidiary case have we known one to be made in our time, which is a very hard nut for the uniformitarian. The extent of these ancient faults is sometimes portentous; the Pennine fault has a throw in some places of 3,000 feet. One near Sedbergh can hardly be less than 5,000 feet. A fault in the Appalachians in America, though it produces no marked effect on the scenery, has so dislocated the rocks that, in the words of an observer, " on one side of a crack, over which a man can stride, the highest of Upper Silurian beds faces the lowest of Lower Silurian." This, according to the author quoted (Campbell, *Frost and Fire*, chap. li.), means a displacement of 20,000 feet (see Bonney, *Story of our Planet*, pp. 317, 318).

I have naturally only quoted a few examples of the effects of volcanoes and earthquakes, and I might continue them almost *ad libitum* to show what takes place when the ground is shaken and dislocated by earthquakes and volcanoes and by the shrinking and warping and sinking of lava currents.

Whatever else is obscure from these facts, one thing is plain, namely, that while it seems impossible to account for the larger features of the earth's surface, its mountains and

valleys, its fiords and clefts, its inland cliffs and escarpments, its lakes and cirques, by the slowly operating handiwork of diurnal erosion, there is not one of these phenomena which is not being repeated on a smaller scale wherever earthquakes and other subterranean movements are in progress. Here, then, is a *vera causa* to which the old masters turned, and from which the followers of Lyell have turned aside in pursuit of a mere will-o'-the-wisp.

What differences there are, are differences of degree and not of kind. During human memory we have had no records of upheavals of mountain chains, no turning up on end or reversal of strata many hundreds of feet thick, nor the forming of tremendous faults such as those above quoted. These and other phenomena point to paroxysms of energy which have marked the earth's history before the days when chronicles were kept; but we cannot doubt if we follow induction that the larger phenomena are to be explained by others whose effects are repetitions on a smaller scale of what once happened, and whose eloquent testimony cannot be got rid of by appeals to forces whose competence is repudiated by those physicists whose laboratory is not limited to their imagination.

Let us now turn to the phenomena we have to explain, and see whether the older prophets of geology or their successors are the more reasonable. The fashioning of the earth's surface contour may probably be divided into two periods—one in which the crust was a mere slag on the cooling molten globe, like the surface of the moon and that of lava currents, and the other when this surface had by different processes been denuded and replaced by stratified beds of various kinds. The effect of subterranean movements upon these different surfaces was no doubt somewhat different. The stratified beds are held together by a continuous structure, enabling them to be folded like the leaves of a book into curves and bends, in which a large number of parallel layers are bent together just like a pack of cards is in the hands of Sandow, and wherever we can examine mountainous and broken country we readily find examples of this process of folding and bending in which a number of the beds have been bent or folded or flexured together and in the same way.

Suess, whose masterly work is a storehouse and arsenal of

geological facts, is of opinion, and seems to state it without qualification, that the contour of the earth's surface is due to two causes only—either to the squeezing of its various layers into a smaller space by some tangential force acting horizontally, and by which these layers are puckered up into flexures of different kinds, or by subsidences of the ground ; and he attributes this squeezing and flexure to the shrinkage of the earth's crust as a whole, due to its cooling. I am not sure that this cause has not been exaggerated. The crust of the earth has long ago been so much cooled down that its temperature, except when under the influence of diurnal causes, is now apparently fairly constant, and this seems to be also the case with that of the crust for a long distance from the surface. I do not wish to be dogmatic on a difficult question, but it does seem to me probable that whatever shrinkage is now taking place in the earth's sphere is for the most part too low down to affect its surface as much as some have argued.

At all events it seems fairly plain that a great many of the inequalities in that surface are due to local causes rather than to the shrinkage of the whole sphere. What the local causes are which now cause the phenomena of volcanoes and earthquakes, and which once probably did a good deal to fashion mountains and valleys, this is not the place to discuss They are probably manifold, some mechanical and some chemical. One of their results, which seems attested by our experience, is that the earth has not been shrinking everywhere, but that elevatory and perhaps explosive forces have been at work in many places.

This seems to follow from the disturbances themselves being so local and not affecting large stretches of horizontal beds close by. Thus, for instance, in the case of the Himalayas when compared with the great Indian plain, etc., the Urals when compared with the horizontal beds of European Russia, etc., and it may be due to explosions of steam or to the workings of chemical forces in certain localities. Whatever the cause, the result in many cases is, as I have said, that the various layers of the stratified beds are, in some cases, puckered and wrinkled and bent and doubled, and are thus raised into arches or domes or sunk down into hollows, the former being known as anticlinal curves and the latter as

synclinal ones. Sometimes they are single, sometimes arranged in a series of wave-like contours, like the successive waves and hollows in the Bay of Biscay when the sea is uneasy. Sometimes the puckers may be very small and sometimes very large, sometimes close together and sometimes far apart. They form the simplest kinds of mountain and valley structure, and I do not know of any one who attributes *the kind* of mountain and valley structure which coincides with the rolling contour of the beds as of anything but subterranean origin. Sometimes these successive folds are more or less parallel to each other and at other times they radiate from a common centre, as in many mountain chains, but it is quite plain that the mountain and valley in such cases are parts of one structure and correlative to each other like the crest and trough of a wave are.

There is naturally an infinite variety of curve in the possible contours of valleys and hills when due to the variation in the curvature of the folds and curves of the surface beds of the earth.

Lapworth has described the position in its simplest form : " Draw a section," he says, " of the surface of the lithosphere along a great circle in any direction, the rule remains always the same : crest and trough, height and hollow, succeed each other in endless sequence, of every gradation of size, of every degree of complexity. Sometimes the ridges are continental, like those of the Americas ; sometimes orographic, like those of the Himalayas ; sometimes they are local, like those of the English Weald. But so long as we do not descend to minor details we find that every line drawn across the earth's surface at the present day rises and falls like the imaginary line drawn across the surface of the waves of the ocean. No rise of that line occurs without its complementary depression ; the two always go together, and must of necessity be considered together. . . . The unit is always made up of an arch-like rise and a trough-like depression, which shade into each other along a middle line of contrary curvature. It resembles the letter S or Hogarth's line of beauty, and is clearly similar in form to the typical wave of the physicist " (Address to Geol. Sec. Brit. Assoc., 1892, p. 8).

"In plains, again," says Lapworth, "as a general rule, the geological folds or crust warpings are so broad and open that they may be scores or even hundreds of miles from crest to crest, and so gentle that the strata in any single section appear practically horizontal. In *mountain regions*, however, the folds are closely packed together; and in extreme cases the rock sheets are folded up into mighty crust wrinkles, ridge succeeding ridge higher and higher above the sea level as the central parts of the mountain ranges are approached" (Lapworth, *Geology*, p. 70).

As I have said, every geologist attributes *some* of the valleys and hills to this process of plication and folding, due either to a shrinking surface or to some upheaving force under the ground. I will quote a few instances.

Speaking of the formation of certain valleys, Dana says: "The plication of the earth's crust produces alternating depressions and elevations unless the folds are pressed together into a close mass. The depressions are synclinal valleys. . . . Beside synclinal valleys there are also *monoclinal* valleys. . . . In addition there are wider depressions lying between distant ranges of elevation which were produced through a gentle bending of the earth's crust (made up of plicated strata or not), and these great valleys or depressions (like the Mississippi and Connecticut valleys) may be called geoclinal, the inclinations on which they depend being in the mass of the crust and not in its strata" (*Manual of Geology*, p. 722).

Desor divided the main longitudinal Alpine valleys into synclinal valleys or troughs, called *mulden* by the Germans, and isoclinal valleys, called *couches* by the natives of the Jura; while Escher added anticlinal valleys, like the Justithal, east of the lake of Thun. In regard to synclinal or trough valleys, Desor explains how in the complex of the Alps their original form has been largely disguised or lost. He mentions as examples of such valleys that of Chamouni, separating the *Aiguilles rouges* from Mont Blanc; the Urserenthal, which extends from Val Bedretto between the St. Gothard and the Tessin. The Engadine is a wider example, and especially is the synclinal structure noteworthy at the lake of Suvretta. These synclinal valleys sometimes alter in contour in different places, thus the valley of Urseren becomes wider near the

Furka and deeper in the Wallis (Desor, *Der Gebirgsbau der Alpen*, pp. 74, 75).

Lyell says : " In the Swiss Jura that lofty chain of mountains has been proved to consist of many parallel ridges with intervening longitudinal valleys, the ridges being formed by curved fossiliferous strata, of which the nature and dip are occasionally displayed in deep transverse gorges called ' cluses,' caused by fractures at right angles to the direction of the chain " (*Elements*, p. 55).

" In certain cases," says Haydon, " as in the Jura mountains and in Western America, the compressed rocks have not been torn and shattered, but have been thrown into parallel undulations, thus forming a succession of long saddles and intervening hollows. These intervening hollows, synclinal valleys as they are called, form the natural channels of rivers " (Philipps, *Geol.*, p. 142 ; see Haydon, *Reports U.S. Geog. and Geol. Survey of America*).

Speaking of the Jura, M. G. la Noë says : " It is easy to verify the fact that the principal rivers there, the Doubs, the Ain, the Dessoutre and the Vedersun, etc., run for the greater part of their course in synclinal folds ".

Certain districts, he says, consist of a more or less parallel series of folds, such as the district between La Perche and Artois. " On y observe," he says, " plusieurs ondulations parallèles largement espacées auxquelles correspondent autant de vallées synclinales qui, par suite du léger plongement longitudinal des plis vers la mer, ont déterminé la création, suivant leur emplacement, des cours d'eau principaux de la contrée " (Seine, Somme, Canche, Anthie, etc.).

Let us continue, however. It seems a plain and elementary truth that when we are dealing with masses of rock of a more or less rigid character we cannot crumple and bend them *ad libitum* without fractures occurring. These fractures will occur where the lines of weakness are to be found.

Hopkins examined the problem in a paper published in the *Transactions of the Cambridge Phil. Soc.*, vi., pp. 1-84. In this paper he deals with the inequalities of the surface due to upheaving forces beneath the ground. He assumes the elevatory force to act under portions of the earth's crust of considerable extent and at any assignable depth, either with uniform in-

tensity at every point or in some cases with a somewhat greater intensity at particular points; and he further postulates that the elevatory force, whatever its origin, acts upon the lower surface of the uplifted mass through the medium of some fluid which may be conceived to be an elastic vapour, or in other cases a mass of matter in a state of fusion from heat, and the mass is supposed to be free from joints and plains of cleavage. " The first effort," he says, " of the elevatory force will be to raise the mass under which it acts and to place it in a state of *extension*, and consequently of *tension*. If the increase of intensity in the elevatory force is very rapid, so as to become an impulsive force, it would not be possible to calculate its dislocating effects. If the intensity and the consequent tensions be supposed to be continuous until the mass ruptures, producing fissures and dislocations, it would be possible to calculate the dislocating effect.

" If the elevatory force be *uniform* the extension, and therefore the tension, will be entirely in a direction perpendicular to the length, so that its whole tendency will be to produce *longitudinal* fissures, or such as are parallel to the axis of elevation. Some of the fissures will be complete ones and will reach the upper surface of the mass, while others will be incomplete and rise to different heights in it without reaching the surface.

" If the elevatory force is not uniform but acts with greater intensity at particular points along the general line of elevation, transverse fractures will be produced in addition to the longitudinal ones."

This explains why so many longitudinal valleys are characterised by fractures. This is the case with the Seine, as has been shown by the French geologists. It seems also to be the case with the Thames.

Martin, in his *Geological Memoir on West Sussex*, suggests that the difficulties met with in making the Thames tunnel were occasioned by a disturbance which exists in the mineral strata along the greater part of the bed of the Thames. Where rent and subsidence have taken place, there remains nothing but loose diluvial and alluvial soil, through which it must be at all times dangerous to venture (*op. cit.*, p. 89).

I have no doubt that a very large proportion of longitu-

dinal valleys have such fissures and cracks in them, the necessary result of the crumpling of the strata. The fractures, as I have said, do not only take place in the hollows or troughs, they also occur along the ridges, downs or wolds which bound them, and in such cases give rise to what Dana calls monoclinal valleys. He says: " When the back of an anticlinal mountain is divided the mountain loses the anticlinal feature, and the parts are simply monoclinal ridges. As the anticlinal in the progress of its formation is almost sure to have its back fractured from the strain on the bending rocks, the removal of the upper and central portion making a broad valley in its place is a common fact " (*Manual of Geology*, 2nd ed., p. 646). These monoclinal valleys of Dana are what others call anticlinal valleys. In some cases, as we shall see, the wide valley has been formed by the gaping apart of the two walls of the fissure, to which I shall refer presently. In some cases, however, it is probable that more than one such fissure exists, and that this has had an important result, there having been a subsidence of the area between the cracks.

These subsidences are especially frequent in limestone districts, in which there would seem to be a large number of hollows under the ground through which rivers sometimes flow, and it would further appear that it is by the sinking of the ground over these hollows that in many cases valleys have been formed or deepened. At all events I know of no theory which can otherwise account for certain valleys with caves containing the bones of pleistocene animals long covered in by stalagmite in their flanks. They occur in the perpendicular faces of the cliffs bounding them at various heights from the ground, where they would not be accessible to the beasts which certainly inhabited them if they had been at their present depth.

The other alternative is that these valleys, which are now chiefly dry valleys, have been excavated by meteorological agencies *since pleistocene times*, although they are at present not being deepened at all, but have been for a long period gradually being filled with débris, a view which seems preposterous.

The fractures along the line of strike of the beds, whether along the ridges or along the synclinal hollows between them,

are not the only rifts and fissures which elementary mechanics compel us to invoke when we have ridges and downs and long mountain chains being raised up from one level to another. The question was examined mathematically, as we have seen by Hopkins and settled for all time. He settled what is now, in fact, an elementary truth. It is hardly possible to raise up long mountains or hog-backed or whale-backed ridges (such, for instance, as the North and South Downs, whose curved strata show that they were so raised) without causing a series of transverse fissures at right angles to the line of the ridges. This is not a mere speculation, a mere possibility, it is, as I have said, an absolute mathematical necessity of the position, and was seen to be so by the old masters. They had noticed that in almost every part of the world the rivers which are running down the main valleys suddenly, in many cases, turn aside from their open and easy route to the sea and pierce at a greater or less angle the masses of crystalline mountain which bound their beds in what seems an unaccountable fashion. They never dreamt that a geologist so widely known as Ramsay would have dared to suggest as a possibility that the rivers had from choice run themselves against these hard buttresses quite arbitrarily and needlessly and out of sheer playfulness, and continued to do so until they had drilled chasms through them when there was no necessity of any kind, and when they had neither the force nor the tools to do such work. Nor did they dream that others, almost as famous, would propound the still more daring theory of Beete Jukes, which we have analysed in a previous chapter.

They were content to be told by the mathematicians and the geologists who knew something of mechanics that the whole matter was perfectly plain and simple and needed none of this transcendental machinery to explain it. The explanation is that the rivers first ran through these side drains because these side drains were there ready made for them and proved to be more easy routes than the longitudinal valleys; and secondly, that the side drains were there because it was impossible to raise up the mountains at all without at the same time making the clefts through them. The one process was an absolute corollary of the other, and if the mountains were ever raised up at all (and will any one deny that ?) the fissures

must be there, and they must occur not arbitrarily, but just where the lines of transverse weakness are to be found. It is by this simple creed that I abide. It was taught to me a long time ago by the Gamaliels at whose feet I sat, and I have seen no simpler and no completer theory, for I hold those of Ramsay and Jukes to be simply midsummer madness.

If we detach ourselves from this last epidemic we shall be constrained to conclude that, as a rule, longitudinal valleys are due primarily to flexures, in many cases culminating in fractures, and exist mostly in synclinal curves, while transverse valleys are due to transverse fractures, necessarily caused by the upheaval of the Downs or other ranges bordering the longitudinal valleys.

When we have to face a great complex like the Himalayas or the Alps, where the fashioning of the contour has been due not to one but to many upheavals, and where the up-heaving forces have acted in various directions, the problem becomes complicated, and we no doubt sometimes even get synclinally arranged strata forming the high ground and anticlinally formed strata in the low grounds; but this last is an exception, the rule is as I have stated it.

If we carefully examine these transverse valleys we shall find that they consist of two different kinds altogether. In one the strata on each side of the gap exactly correspond and are opposite to each other. In the other case there is no such correspondence, but there has been a fault by which the strata on one side of the valley have either been raised up or let down, so that the corresponding beds are not opposite to each other. The former are generally of a V-shaped outline, more or less modified by subsequent erosion. This, of course, has acted more powerfully among the softer rocks, and this gives such valleys a varying contour according to the materials in which they are cut. In chalk they are soft and flowing, like those of the cooms on the Downs. Where the rocks are crystalline and hard, the transverse valleys are sharply out-lined, with often zigzag courses and clean-cut sides and contours.

I must here enter an emphatic protest against a notion which widely prevails, that a valley due to fracture must

show something in the nature of a fault, namely, a want of symmetry between its two flanks. Nothing of the kind is necessary, or would be even possible, in the case of many fissures caused in the way I have mentioned. If the ground cracked and then began to gape the openings formed would, in the majority of cases, have perfectly symmetrical sides, each stratum being immediately opposite to the bed to which it was once joined, and it would only be in exceptional cases that anything in the shape of a fault would be forthcoming at all. Hence those geologists who have always argued in favour of these valleys being valleys of erosion, because of the symmetry and continuity of the beds on either side of them, have ignored the conditions of the problem altogether. The fissures we refer to as constituting transverse valleys are, in fact, nothing more nor less than crevasses ; they are made in the same way, due to similar strains, and are, in fact, the counterparts of the crevasses in Alpine glaciers ; they differ only in that they occur in solid rocks instead of in ice, and are permanent instead of transient. A crevasse in a glacier is caused by the state of tension created when the mass of ice is forced to move over some hillock or other obstruction which gives it more or less of an arched contour. A similar strain on a sufficient scale will cause a crevasse in the toughest rocks.

It is a delusion, therefore, which is continually being pressed upon us by the advocates of erosion as a cause of valleys, that in so many cases there is no fault in the line of the valley, but that the strata on each side correspond. The fact is that in V-shaped fissures in ice and lava, which are the principal cases we can test the problem by, and in the great bulk of earthquake fissures, there is no faulting at all, nor is there any mechanical reason why there should be. On the contrary, the strain merely forms a crack which is opened into a V-shaped fissure by the greater strain of the upper layers of the rock. The V shape and the correspondence between the two sides is and must be the rule in fissures caused in a contracting mass of rock. This argument of the erosionists is therefore singularly wanting in force. Let us go on. It is plain that what has happened to lava when under the stress and strain caused by contraction and cooling has occurred repeatedly from other stresses and strains in the

consolidated rocks. To use Prof. Bonney's expressive phrase : " They have been snapped across under great strains, and the broken ends are now separated by hundreds, sometimes even thousands, of feet " (*The Story of our Planet*, p. 234).

Having given a general view of the theory which, in my opinion, alone accounts satisfactorily for the great mass of longitudinal and transverse valleys, I will now quote a number of examples from different places as types, and will also quote the opinions of my "masters" upon them, and will begin with Martin, who discussed so ably the transverse valleys of the chalk long ago, and whose name is seldom seen in our brand-new manuals of geology.

Martin states his case thus : " The strata which compose these basins, previously in a horizontal position, suffered disruption ; and in the act of basining (whether by the elevation of the sides or the subsidence of the central parts is not now material) all their parts were deeply and extensively fissured in an order correspondent with that act, producing, with the help of diluvial action, a system of longitudinal and transverse valleys answering to the double inclination of their fractured masses, and a consequent removal of the broken materials brought within range of the denuding force " (*Geol. Mem. of a Part of West Sussex*, p. 59). He argues that the strata were once continuous from the Channel to the Thames. To use his words : " The convexity extends from the bottom of the English Channel to the bottom of what is called the London basin. This convexity may be likened to a dome, and the loss of a part of the crown of the dome is the Weald vacuity. In other words, the commencement of each basin is in the anticlinal line of the Weald valley. From this point all the strata begin to slope. Both basins, therefore, may be said to be entire in a part of the Wealden, although they have lost a part of their rims in the chalk and the glauconite (*i.e.*, the greensand) " (*ibid.*, p. 61). He argues further that when this dome was raised and the strata were broken either by the elevation of the central parts or the depression of the lower ones, it would necessarily give rise to transverse fissures. Of these fissures he says : " The principal transverse fissures, some of which were destined to become river channels, have a remarkable correspondence on each

side of the valley, particularly in the chalk, and in several instances are directly opposed to each other, which would not have happened without a simultaneous action and common consent and continuity of parts. The direction of a rent would be ruled by the density and tenacity of the different parts of the stratum ; occasionally deviating from the straight line, it might be lost in one part and taken up and carried on by another giving less resistance. The coincidence is therefore the more remarkable, and proves not only the continuity of the chalk strata at the moment of convulsion, but also their uniform density and strength. Of the valleys thus opposed to each other by the continuity of the great transverse fissures, the most remarkable are the defiles of the Arun in the South, and the Wey in the North Downs; the vale of Leatherhead, or the Mole, and that of Findon, or the Worthing Road ; of the Adur and Smitham bottom, or the pass of Merstham and Croydon. The Ouse is also opposed to the Darent, and the Cuckmere to the Medway " (*ibid.*, p. 61).

" The simplest state in which the valleys of fissure exist is that in which, after the rent was formed, the opening has been enlarged at first by diluvian and since by meteoric abrasion. Such, for the most part, are the great river outlets through the chalk strata, and many of the water-courses through the rocky beds " (*ibid.*, p. 63).

The next in order is that in which the rocks have dropped into rents beneath as in the cooms, forming valleys of simple subsidence. These are to be found everywhere where the tabular disposition is favourable to observation. The greensand country from Warminghurst to Bedham Hill furnishes many picturesque examples of this modification of the fissure.

" A third modification is that where the opening of a rent below has caused not only a similar displacement of the parts above, but has also brought down a portion of them, so that instead of a simple gaping of the strata we see them perhaps on one side of the fissure leaning away from it, on the other dipping sharply into it " (*ibid.*, pp. 63, 64). " Wherever the stony courses have been opened in the weald district, fractures, contortions and variable dips are to be observed. Long and irregular rents pass transversely through them and take up

the streams, which, from the rarity of springs in this forma-
tion, are for the most part only winter ones, and convey them
to the greater river outlets " (*ibid.*, p. 65).

The fractures are sometimes found to terminate in notches
of denudation, where, by the removal of the broken parts, the
beds beneath are brought into view. An erosion of this kind
exposes the gault far up in the malm at Bignor Mill; and the
northern part of Petworth stands in a notch of the lower
greensand of the same kind. Another well-marked example
of the joint operation of fracture and erosion is to be found at
Pulborough, where a coom carrying a small stream extends
from the escarpment of the ferruginous sand, through New-
Place farm, about three quarters of a mile, and terminates in
a notch in the lower greensand, a valley of weald clay at
Bromer's Hill about a quarter of a mile in length. Another,
with good examples of valleys of subsidence, subordinate or
branching off from it, may be traced from the hamlet of
Nutbourne, through Bury Grove and out at Crowell.

Every water-course intersecting the basseting edges of the
strata is in fact a notch or crack enlarged by comminution
and diluvian action, *the open extremity of a fissure.* This applies
to the chalk as well as to the minor formations; every river
outlet is a crack in its basseting edge. If the angle of subsi-
dence is acute, the fissure shows as a mere gap; if the stratum
is more horizontal, and spreading over a wider surface, the
fissure is a long valley or succession of valleys (*ibid.*, pp. 64, 65).

Martin quotes the valley of the Arun as a typical specimen
of a transverse fissure valley. In the upper part, where it
passes through the Shanklin sands, " the character of a
fissure is most apparent on the eastern side, where the greater
compactness of the materials seems to have preserved them
from comminution and erosion. Near the verge of the gorge
large masses are found sloping towards the river, and farther
back the entire bed drops at a very acute angle, and is now
quarried near the spot at which it is disjointed from the main
body. The whole slope of this side of the gorge is formed by
the dropping down of a large angle of the stone, from the loss
of its support. . . . A similar disposition is to be observed at
the entrance of the defile which carries the stream that runs
into the Rother, between Petworth and Byworth. In the

quarries in the Rectory grounds the stone may be observed
sloping rapidly easterly or down towards the stream, and the
fractures answering to this inflexion have been disclosed at
the top of the hill and about the Rectory Home " (*ibid.*, p. 66).

Returning to the Arun valley, the western side of the
defile at Stopham does not correspond much with the eastern,
except when the most compact beds of greensand are opposed
to each other. After the Arun unites with the Rother
they pass through a broad expanse of alluvium, resting appar-
ently upon gault, and outlines of that stratum are found
rising from under it in several places. " Nor are other evi-
dences wanting of fissure and of subsidence in this part of
the river course. The longitudinal valley in which the Arun
receives the Rother, and which carries the united rivers round
by Pulborough, is marked on the north by a sudden dip into
it, extending from Fittleworth to Chiltington Common. This
is particularly visible at the turnpike gate at Stopham and in
Pulborough-church hollow, and, on the south side, at the
blacksmith's shop at Cold Waltham, from whence the gault
extends through the peninsula of Hardham. At the latter
place it drops in towards the river at an angle of twenty or
thirty degrees " [1] (*ibid.*, pp. 69, 70).

" The chalk gorge, as might be expected, furnishes a fine
example of fissure, enlarged and modified by erosion. A
visible backward bearing of the rocks on each side is not a
necessary part of the evidence of fissure, because in the pro-
pagation of a rent upwards through strata of various texture
and tenacity much irregularity would ensue in their disposi-
tion ; but here there is direct testimony of divergence, and
the enlargement of the opening by diluvian action is indicated
by the gradual slope on either side " (*ibid.*, p. 70) " It is
impossible to view the structure of this outlet, and the evi-

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[1] The disruption and subsidence is visible by the roadside, within a few yards
of the turnpike gate at Stopham, and the gault is seen emerging from under
the diluvium upon which Stopham House is situate. The longitudinal valley
in which the Rother here takes its course and in which it is united with the
Arun is one of subsidence, apparently connected with the fissure of Jacquet's
Hill and the vale of Greenhurst, and is a continuation of the greensand basin
or trough, the apex of which is Warminghurst. The outlying masses of gault
of Wiggonholt, Hardham and Tripp Hill are all in the line of this subsidence
and carry on the marks of the disruption towards Duncton Hill.

dences everywhere of disturbance and convulsion, without speculating on their mode of operation" (*ibid.*, p. 72). "If it be proved that these masses are deeply fissured it is also proved that they have been in motion, and the formation of such a channel in a direct line through such heterogeneous materials must have been the result of a simultaneous movement of the whole, let the moving power be what it might. That this operation was coeval with the catastrophe which left the material features of this part of the world such as we see them is apparent from a collective view of the concomitant phenomena, and whether the Sussex and Surrey hills, with their accompanying strata, were severed by the disruption and dispersion of the intervening parts or simply by the sliding down of the whole mass, or the joint effect of these causes, the slightest inclination of the basis upon which it must be supposed to rest would be sufficient to open the fissures in the direction in which we see them" (*ibid.*, pp. 73, 74). Martin quotes Murchison (*Geol. Trans.*, 1826) for evidence that the dip of the beds, which at the Alton chalk hills is scarcely perceptible, increases towards the South Downs until it reaches an angle of ten or fifteen degrees, and he quotes other instances and conclude thus : " It is obvious that every change of dip and inclination or lateral bearing must be attended with fissure, or the contortion which is made up of a series of minute fractures ." Thus, if the inclination has increased from ten to fifteen degrees in any given distance, fractures commensurate with that increase must somewhere exist (*ibid.*, p. 74, note).

" The next transverse fissure," says Martin, " that can be plainly seen is the defile by which the Worthing Road traverses the South Downs by Findon. It is opposed to the valley of the Mole in the North Downs, but it carries no water till it issues from the Shanklin sands at Ashington Malt House. About and below Findon great quantities of chalk flints have been washed into it, so as to give it the appearance of a river bed or the course of some ancient torrent. The comminuted chalk at the upper or northern end of this disruption has been well exposed lately in some deep cuts made to widen and improve the road. . . . A westerly bearing (away from the fissure) is observable

in the stone quarry behind Ashington Malt House. This rent divides the chalk between the Arundel and Shoreham outlets into two great masses, as those of Piecomb and Saddlescomb do that between the Adur and the Ouse. The Adur or Shoreham fissure is very quickly distributed, and by one of its larger branches gives a river to this district called the Bines. . . . The Shoreham fissure is very nearly opposed to the depression in the North Downs between Croydon and Reigate, called Smitham Bottom. . . . The fracture giving rise to the latter cuts through the Shanklin sand between Red Hill and Red Stone, upon the Brighton Road, giving transmission to a branch of the Mole which rises at Merstham. At this place, besides giving an exit to the stream, it cuts off the banding angle of the Nutfield fuller's-earth beds, which thin out upon Red Hill, on the opposite side of the ravine. If this ravine did not exist, the stream, left to its natural course, would run along the foot of the chalk hills and out at Reigate Heath " (*ibid.*, pp. 75, 76).

Martin refers to the part of the Weald valley from **Mary Hill** eastwards as an unmistakable longitudinal fissure which, so far as the subjacent strata are concerned, ceases near Poynings, while in the chalk it divides into two branches and forms the depressions of Piecomb and Saddlescomb. A crevice from the latter is the well-known fosse of the Devil's Dyke, which is a good specimen of the manner in which a rent may be enlarged by diluvian and meteoric action. Minor examples of the same kind may be seen at West Burton Hill, between Stoke and Rackham, and in other parts of the South Downs (*op. cit.*, p. 81).

Again he says, speaking of some of the transverse valleys, that, while originally fissures, they have been so enlarged by watery action as to have lost all vestige of their origin, except their transverse direction. Among consequences of the latter action he speaks of the enlargement of the river crevices, and the rounding off of the angles of fracture, the scooping out of the clay, the filling up of gullies, and the entire removal of broken masses as sufficiently obvious causes. " To the eye of the practised observer the Weald valley presents the appearance of a great water channel after a flood ; some parts of it clean and clear from all encumbrance, others loaded with

drift; the banks in some parts torn clean away, in others heaped up with rubbish " (*ibid.*, p. 84).

" A double elevation or a force operating obliquely to the planes of the strata was the cause of the cross fracturings which opened the river courses and broke up the contents of the Weald valley. . . . The junction of the chalk with the glauconites, which is at the level of the sea at Beachy Head or on the Kentish coast, may be computed to have risen 200 feet above it under the Alton Hills. The river and other transverse fissures answer to this elevation, and the obliquity is shared by the several intervening masses, though it may be in very unequal proportions. In tracing the marks of this operation it seems natural to look for an elevation of the western side of every material fissure, and it is not improbable that this is often, if not always, the case. Thus the malm rises higher on the western side of the Arun than on the eastern. The bottom of the remarkable bed of greensand at Bury stands twenty feet at least above the river, while the corresponding part, with which it must once have been continuous, east of the river at Amberly, is lost under its alluvium. Had the parts remained in juxtaposition this would have been only a *fault*, but in conjunction with the lateral divergence it becomes a gaping and a river fissure " (*ibid.*, p. 97, note).

I have quoted Martin's observations at considerable length, not only because I think them perfectly sound, but as showing what sane and sensible men the old geologists were in dealing with these difficult problems. Similar views were held and applied by others.

Scrope calls the longitudinal depressions valleys of elevation or anticlinal valleys. " Where such anticlinal valleys are on a large scale, as the instance of the Weald of Kent, transverse fractures appear to have been also formed in the subsiding strata; but these will have remained, more or less, narrow crevice-like gorges, since no subsidence can take place away from them that is in a direction coincident with the axis of elevation. These transverse chinks or gaps are frequently the channels through which the drainage of the interior valleys has been effected, while they have been enlarged and have lost the angular roughness of their fractured edges by denudation or meteoric abrasion. Such are the gaps in the chalk

escarpment of the Kent and Sussex valley, through which flow the Wey, Mole and Medway towards the north, and the Arun, Adur, Ouse and Cuckmere towards the south " (Scrope, *On Volcanoes*, p. 213).

Fitton says : " The mode in which the drainage of the north-west portion of the great valley of the Wealden is effected, and the different manner in which the streams escape in this part of the country from that of their egress through the North and South Downs, support the hypotheses of Mr. Scrope and Mr. Martin that the gorges themselves were not produced by simple denudation, but at least prepared by antecedent fissures cutting entirely across the Weald " (*Geol. Trans.*, 2nd ser., iv., p. 150).

Mantell (*Geol. of S.E. England*, pp. 352, 353) says that the transverse valleys of the North and South Downs, although they are now river courses, obviously originated in disruption, for the strata in every instance which he observed diverged from the line of fracture.

" At Malling Road near Lewes is a deep ravine called the Coombe—the lower chalk forms the lower two-thirds of the cliff, the upper portion being composed of the *flinty chalk.* The southern side of the valley, on the contrary, is made up entirely of the *flinty chalk*, proving that the valley coincides with a line of fault. The flinty chalk continues with a slight southern declination to near Southerhouse corner, when *the lower chalk* is again brought to view, and is seen tilted up in the chalk pit near the turnpike gate and dipping at a considerable angle towards the *north*, the chalk marl appearing beneath it. This section is a beautiful illustration, on a small scale, of the faults and dislocations produced by elevations and subsidences, and the Coombe is a fine instance of a valley formed by the derangement of the strata."

Bakewell says : " Transversal valleys, or those which cut through mountain ranges nearly at right angles to the ranges they intersect, may have been originally fissures or openings, made either at the period when the ranges were elevated, or subsequently by the same causes that have rent and displaced the secondary strata. These fissures may have been afterwards widened by the erosion of water " (*Introduction to Geology*, pp. 385, 386).

Samuel Woodward in his *Outline of the Geology of Norfolk* (pp. 1, 2) arrived at the same conclusion in regard to the chalk valleys of Norfolk. " These," he observes, " are *valleys of disruption;* that is, they were formed by the elevation of the chalk and its consequent fracture, as is evident from the strata of chalk and flints on each side, the valley being now found to decline from the line of elevation."

He further observes that from the high ground between Lopham Ford and Brancaster, where the Waveney and Ouse rise within a few feet of each other and diverge, the rivers of Norfolk flow east and west according to the inclination of the chalk, and that the valleys of Norfolk are valleys of elevation due to the elevation and fracture of the chalk.

Speaking of the rivers of Suffolk the Rev. W. B. Clarke writes : " How far these river channels may be due to dynamical action it is difficult to determine, but I am of opinion that when they are studied with reference to proofs of violent derangement in the north, east and west corners of Norfolk, and the almost unequivocal testimonies of disturbance on the coast of Suffolk, there is sufficient reason to assume that the drainage of Norfolk, Suffolk and Essex has been induced by a violent strain acting from below and throwing the whole mass of the country into a position by which 1,200 square miles of Norfolk and 220 of Suffolk are drained by Yarmouth Haven, and about 2,000 square miles of the latter county at the south-east corner " (*Geol. Trans.*, 2nd ser., vol. v., p. 359).

The same writer, speaking of the deep transverse valley which intersects the mountain called La Roule, a little to the east of Cherbourg, says : " The east escarpment from 300 to 320 feet in perpendicular height slopes gently to the south as far as the commencement of the slate, where the slope becomes extremely rapid into the valley of the Divett. The west side of the ravine presents similar features but in a reversed position, the escarpment of quartz rock crowned by the telegraph being to the south, and the slope to the north with a very gentle inclination till it reaches the sea level. . . . This ravine agrees precisely with the strike of the runs of pure white quartz which intersect the strata from south-east to north-west, which corresponds with the mechanical effect of eleva-

tion. This is not a local but a widely-extended operation, and probably connected with the transverse break through the beds of hard chalk at Corfe Castle in Dorsetshire." He argues that the most striking phenomena on each side of the western part of the English Channel is due to the same forces. *Inter alia*, he mentions the existence of a series of faults in Dorsetshire, ranging in a linear direction with the elevated beds of the Cotentin, as well as another transverse series on the coasts of England and France, leading to the view that the tract now covered by the English Channel between Cape La Hogue and Portland owes its existence to the denudation of the deposits intermediate between the slate and the oolites in the direction of the similar but minor valleys in La Manche, and that the appearances of elevation so extensively developed on both sides of the Channel are due to one common deep-seated cause, evidence of which is afforded in the traps and granites of La Manche, Britanny, Devonshire, Cornwall and the Channel Islands; and in regard to the view of a previous observer, that the west coast of the Cotentin originated in a fracture, he observes that not only the coast lines of the Cotentin, but of the Channel Islands, Purbeck and the Isle of Wight will be found to have been produced by a series of contemporaneous or successive upheavings of the strata in the same linear direction *at an epoch posterior to the formation of the tertiary deposits* (*Geol. Trans.*, 2nd ser., vi., pp. 566, 567).

Jukes-Browne tries to explain the present course of the Witham by arguing that it was diverted by the production of an anticlinal by an uplift quite in recent times (*Mems. of the Geol. Survey*, Sheet 70).

In regard to some of the chalk valleys of Wilts and Dorset, Buckland says these valleys at first sight appear nothing more than simple valleys of denudation; but the part of the strata composing their escarpments having an opposite and outward dip from the axis of the valley, and this often at a high angle, as near Fonthill and Barford in the vale of the Nadder, and at Oare near the base of Martinsell Hill in the vale of Pewsey, obliges us to refer these inclinations to some antecedent violence, analogous to that to which I have attributed the position of the strata in the enclosed valleys near Kingsclere, Ham and Burbage. Nor is it probable that without

some pre-existing fracture or opening in the lofty line of the great chalk escarpment, which is here presented to the north-west, the power of water alone would have forced open three such deep valleys as those in question without causing them to maintain a more equable breadth, instead of narrowing till they end in a point in the body of the chalk (*Geol. Trans.*, 2nd ser., ii., pp. 123, 124).

In his paper on the " Geology of South Devon," Godwin-Austen tells us how " along the line of the Haldons there is abundant evidence of a great rise of the beds to the east. There is a difference of at least 800 feet in the present position of the greensand beds at Haldon and Bovey, which were once continuous. . . . Subsequent to this period of denudation, *i.e.*, that during which the local gravels are distributed, and under the same conditions with the fissures of the limestones, were formed those great fractures, along one of which the Teign flows as far as Chudleigh, also the parallel valley from Bovey to Moreton, and the origin of which was evidently connected with the numerous abrupt movements that produced the deep valleys in the granitic region of Dartmoor, and which being but little above the level of the sea can have no great antiquity, as they are unoccupied by any sedimentary accumulations.

" The lower part of the course of the Teign, like that of the Exe and many other streams, is through a valley of excavation along a line of fault. Its course is due east and west, and it will be seen that the mouth of the Teign is the only point along the coast section of the new red sandstone where the strata have an anticlinal dip to the north and south. Higher up the valley and on the north bank opposite Combe-cellars is a fault which, for more than half a mile in an east and west direction, brings up the slate in vertical juxtaposition with beds of new red sand; the downcast, as usual, being to the west, but the amount uncertain. Nearer Teignmouth the same fault may be traced through some conglomerate beds; and westward it ranges along the foot of the cliff of greenstone opposite Hackney clay-cellars by Newton; and from Bradley to Holbeam Mill it traverses a great mass of limestone in a zig-zag course, owing to the rock having yielded along its two sets of joints. Beyond this it

cuts a line of hill, consisting of slate with trap dykes. The direction of the hill is diagonal to the direction of the fault, which passes through at a right angle, but immediately resumes its course along the valley of the Lemon; still further it breaks the band of limestone below Bickington, then enters the carbonaceous deposits, and may perhaps be connected with some of the east and west faults which, as at Owlecombe, become metalliferous as they approach the granite beneath Rippon Tor.

" At Bickington and along its whole course the displacement of strata, both vertically and horizontally, is very remarkable, the beds on the north side of the fault being apparently shifted to the east.

" The faults in the cliffs at Dawlish range westward in their inland course, and the lower beds of greensand on the Haldons are put at different levels by faults having a like direction and which traverse the superficial beds.

" It appears, therefore, that the more recent disturbances had a general east and west direction. In the description of the granitic region of Dartmoor it has been stated that very many of the lines of hill and valley instead of conforming to the range of the several masses of Cornwall and Devon have courses from north to south, and many of these lines of elevation extend from the granite into the area of the carbonaceous deposits on the north. The Haldon Hills are on a line due north and south, and the proofs of elevation observable along the west slope have already been noticed. It was this disturbance which also opened the north and south joints of the limestone, and parallel with the Haldons is the greensand escarpment of the Blackdowns.

" The course of the Exe as low down as the head of the estuary, where it falls into another line of disturbance, is along a most extraordinary north and south dislocation, which, like the east and west fault already described, has severed beds once continuous, and to a much greater extent; numerous faults with the same direction run through the Blackdown Hills (see *Geol. Trans.*, 2nd ser., vi., p. 433, etc. ; De La Bêche, *Researches*, and also his *Report on the Geology of Cornwall and Devon*). The character which chiefly distinguishes this system of faults, apart from direction, is the

very great amount of vertical movement which the beds to the east experienced."

In regard to the origin of fissures in limestone, Godwin-Austen says : "If we take the fissures of the Chudleigh country as a guide we find that in every instance they have been opened along the lines of joints in the lime rocks. Thus the great mass of débris cemented by calcareous matter, which crosses the quarry at Chudleigh rock like a huge wall of coarse masonry, is the contents of a north and south joint from which the solid rock has been removed on each side. The same quarries afford many analogous instances. All the joints in a mass of arched limestone near Barton have been similarly opened and filled; and these, as well as all the fissures which are to be found along the base and west slopes of the Haldons, as at Orchard Well, Lindridge Hill Wood, etc., suggest that the strata must have been subjected to great tension, which caused them to yield along the joints as lines of least cohesion " (*Geol. Trans.*, 2nd ser., vi., pp. 484, 485).

The valley of the Avon at Bath, again, is also the seat of one of those disturbances to which Sir Charles Lyell alluded when he candidly said that he had " little doubt that the Bath springs, like most other thermal waters, mark the site of some great convulsion and fracture which took place in the crust of the earth at some former period " (Address Brit. Assoc., 1864, p. 64). The hot waters of that city have ever flowed out of a deep-seated fissure, clearly marked by the strata on the one side of the valley having been upheaved to a height of 200 feet above that which they once occupied in connection with those of the other side. When, indeed, we look to the lazy-flowing, mud-collecting Avon, which at Bath passes along that line of valley, how clearly do we see that it never deepened its channel. Still more, when we follow it to Bristol and observe it passing through the steep gorge of hard mountain limestone at Clifton, every one must be convinced that it never could have produced such an excavation. In fact we know that from the earliest period of history it has only accumulated mud and has never worn away any portion of the rock.

The fact is the same kind of phenomena occurs everywhere. " In the Isle of Wight a high ridge of chalk, running

from east to west, forms the backbone of the island and the natural watershed, but the three chief rivers, the Braday-brook, the Medina and Yare, rise to the south of the ridge and run north into the sea *through* this opposing ridge by depressions evidently not made by water running from the watershed, which, as it is, yields only small rivulets. The same kind of thing occurs in the drainage of south-east England. The natural watershed of the oolite and chalk ranges is utterly disregarded by the Teme, Ouse and Welland, all of which rise to the west of these hills, and by means of fractures across them enter the Wash and through it the North Sea."

Murchison says the violent operations which upheaved the Ludlow promontory from beneath *the sea* and threw it into an anticlinal form produced the gorge of Aymestry, and at the same time choked it with coarse detritus, which was thus one of the immediate causes of the formation of the Wigmore lake. " These movements," he says, " also doubtless produced the fissure through which the Teme issues into the low country." Again he says : " The course of the Camlet through the fissure of Marrington Dingle, in a direction precisely the reverse of the ancient lines of drainage, is a very striking proof of how this stream has taken advantage of one of the *last formed* rents and depressions by which the surface has been modified ". Again, speaking of the Aymestry fissure, he says: " From the position, however, and different inclination of the strata on opposite sides, it is evident that this coom has been affected by some of the great dislocations which resulted from the heaving up of the Ludlow and adjacent promontories " (*The Silurian System*, i., 549-551, etc.).

In a paper in the fourth volume of the second series of the *Transactions of the Geological Society*, Sedgwick, speaking of the origin of the Cumbrian Mountains, says that his researches prove that at a very ancient epoch great cracks were formed diverging from the centre of the mountains, accompanied by great changes in the relative positions of the mineral masses on the opposite sides, and, further, that these cracks prepared the way for future valleys. Now the principal valleys of these mountains diverge towards all parts of the circumference from a centre near the high crests of Scawfell, and he con-

cludes that it is probable that great lines of dislocation pass down the greatest number of these valleys. If this hypothesis is admitted, we can advance a step further and point to the probable origin of these great diverging fissures, for the valleys start from a central region which is violently broken, where the dip and range of the stratified masses is unsymmetrical, and which is marked by protruding mountains of granite and syenite (*op. cit.*, pp. 54, 55).

Speaking of the mountain valleys of Cumberland, Hopkins says " the primary effects of the faults has been the valleys in which they occur. The existence of the faults compels the conclusion that the valleys originated in these dislocations, . . . and here we are led a step further by the closest analogy. If the valley of Wastwater originated in a great dislocation, it is hardly conceivable that the adjoining valley of Eskdale should not have had a similar origin. And again, if the valleys of Troutbeck and Kentmere, on the south-west side of the district, have been caused by dislocations, it is difficult to suppose that the valley of Long Sleddale should have been formed independently of a similar cause." He also quotes the valleys of Borrowdale and Langdale and the valley of St. John as teaching a similar lesson (*Proceedings of the Geol. Soc.*, 1848, p. 74).

Hopkins again says of the fault in the valley of Duddon near Duddon bridge, and of another passing along the valley of Hallthwaite, that it is probably to the combined effects of these faults that the valley of Duddon is to be referred. Another fault runs down the valley of Troutbeck and ranges with that part of Windermere lying south of the embouchure of the valley. On the east of Troutbeck also there are dislocations which cannot be doubted to have been connected with the formation of the two striking valleys of Troutbeck and Kentmere (*Proceedings of the Geol. Soc.*, 1848, p. 74).

Professor Phillips says : " Certain great ridges and hollows which limit the drainage of the Lune and its branches were plainly sketched out by subterranean movements. . . . Leck Beck and Barbon Beck were marked out by great faults, while others not in directions of such faults were yet traceable to lines of weakness in rocks, occasioned by joints bearing a determinate relation to these fractures. The conclusion

from the whole being that the main features of the ine-
qualities of the earth's surface were always referable to dis-
placements of the rocks and lines of weakness dependent on
them" (*Geol. Mag.,* i., 230).

Mr. H. Leonard, of the Irish Survey, has given a good
conspectus of the Scotch evidence. He says: "In the
Highlands of Scotland I was greatly struck with *the form of
the ground,* the resemblance to that of portions of Connemara
being complete. The faults here, as in the west of Ireland,
run principally through the *cols* or *maums,* that of Glencoe
passing through the maum at 'Rest and be Thankful'.
The historic valley of Glencoe lies along a line of break in its
schistose rocks, which is very prominently marked from about
the centre to the top of the glen, where the main fault appears
to split into a number, and these are in many cases cut across
by other faults. The glen of the Blackwater lochs is another
example of a valley along a break. In this wild and rocky
gorge the strata are well exposed, so that the data are easily
observed. The bearing almost corresponds with that of Loch
Leven, in which the waters of the glen flow. The Caledonian
Canal, which consists of a series of connected lakes, is also in
the line of a great fault, and very many cross faults were
noted along its route.

"Loch Awe, from the nature of the rock, does not as plainly
tell its tale; but the facts observed in the vicinity of Portso-
nachan, and along its western side, rather incline to the
assumption that it is connected with shrinkage breaks, the
joints having nearly the same general bearing, and indicating
that two sets of master joints aided in producing the physical
features here developed. Breaks or master joints appear to
run into the two glens at the head of the loch."

In the Grampian Mountains the country is, according to
Mr. Leonard, very much faulted and very like Connemara,
and he says that "a visitor who approaches them with the
key furnished from the examination of a similar district
may more truly unlock their history, and perhaps cease to be
fully persuaded that these valleys are to be looked upon as
the results not of subterranean movements but of subaerial
denudation. I may mention," he continues, "that when
travelling through Savoy, Valais and the Bernese Oberland,

I was greatly impressed with the numerous examples presented of valleys excavated in the line of faults. The Arno, Rhône, Visp and their many secondary valleys show countless instances where subterranean movements have aided denudation in carving out the features now existing, and in the narrow rocky gorge where the Aletsch glacier ends the work of denudation combined with shrinkage lines is most apparent."

Sir A. Geikie says of the great rift in Scotland: "This singular straight depression which cuts Scotland in two is a great line or fracture in the earth's crust, probably dating back to an ancient geological period, and subject to repeated movements along the same line. . . . Its very straightness is enough to suggest that the great glen owes its direction to a line of dislocation. I ascertained in the year 1864 that the effects of the fracture, or of one continued in the same line, can be seen along the western side of the Moray Firth, where the jurassic beds of Eathie and Shandwick are thrown down against the old red sandstone" (*Scenery of Scotland*, second edition, pp. 334, 335).

Turning to Ireland, Kinahan says: "It is a popular belief that rivers and streams have excavated the valleys in which they flow. This may be the case in reference to some valleys, no doubt, but in Ireland in general the rivers are due to the valleys, not the valleys to the rivers; the valleys occupying dykes of fault rock or lines of breaks or other shrinkage fissures in the strata or accumulations in which they are situated. West Galway is well adapted to study the connection between breaks and valleys, as large areas of the rocks are bare, or only covered with a few feet of peat, and the breaks in the strata show as if on a map. In this area it is found, with rare exceptions, that even the smallest streams occupy lines of breaks, whether faults or only joint lines; while in the exceptional cases, where a stream has been found to cross over a continuous bed of rock, the stream has had very little effect, in some cases not having been able even to efface the striæ and polish due to the ice of the glacial period."

Jukes argued that the transverse valleys in south-west Cork and Waterford were first cut, and that the longitudinal valleys were afterwards formed by meteoric denudation. Kinahan says that however denuded these transverse valleys

run along lines of breaks, and further west similar valleys are now being cut above breaks by marine action, and he thus explains the maums or gaps across the ridge of hills south of Ballydonegan Bay, whose floors are flat instead of sloping as they ought to be if the result of meteoric action.

He points out that some rivers, such as the Flesk and the Lee, pass by some transverse valleys through which they ought to flow if they had been first carved out. "In Kilkenny, Wexford and Wicklow the rivers occupy valleys which, for the most part," he says, "are oblique or at right angles to the strike of the rocks of the country. The fiords or estuarine portions of the Suir, the Nore and the Barrow are in deep gorges, cutting transversely across the Cambro-Silurian rocks, in which are many bedded eruptive rocks, which, by their displacements, prove the gorges to occupy lines of faults. In the valley of the Barrow there seem to have been successive displacements in different geological times. . . . The Ovoca and its tributaries, which drain the greater part of the county of Wicklow, occupy deep narrow valleys excavated along lines of faults. . . . Along these fault lines dykes of fault rock seem to occur, as can be seen at the Ovoca mines in Glenmalur and in some of the neighbouring fissures like that called Kilmacree Pass. Here the fault rock is shaly flucan with hard ribs, having a stratification transverse to the strike of the rocks of the country.

"The Liffey and other rivers in the mountainous part of County Dublin, although in most portion of their courses they follow breaks, yet in many places they cross continuous masses of rocks that cause cascades and waterfalls. . . . Traces of very recent faults and displacements occur at Hollywood, etc., to one or more of which the diversion of this portion of the Liffey may be due. Westward and south of Bray are the two great maums called the Scalp and Glen of the Downs. These lie in lines of fault, and were probably excavated by marine action aided by ice along dykes of fault rock.

"The Boyne valley has not much the appearance of a fault line, yet, when sinking the foundation for the viaduct at Drogheda, it was found that the calp limestones are thrown down northwards over sixty feet.

" The Bann at Gilford joins the great valley and fault between Coleraine and Carlingford Lough. The Foyle and its tributaries, as well as the rivers to the north-west in the county of Donegal, manifestly follow dislocations in the strata, some of them apparently very recent faults. . . . It is evident that the valley of the Shannon in general coincides with lines of breaks, although in some places continuous breaks cross it, being apparently diverted by banks of esker gravel."

Kinahan describes the river valleys of Cork as covered and disguised by alluvial deposits, and observes that consequently the connection between the valleys and faults is not apparent, but on an examination of the cliffs it is found that there is not one of the fissures extending to the coast-line which is not connected with a break or fault in the underlying rocks. Furthermore, in those portions of south-west Cork where mining operations have been carried on, slides, heaves or cross courses have been proved under every transverse valley, ravine, fissure and river or stream course that has been mined under; and such faults suggest that all the transverse river valleys in south-west Cork are connected with lines of breaks or faults (*Valleys, Fissures,* etc., pp. 173, 174).

Again Kinahan says : " In West Galway and Mayo narrow fissures running along lines of faults and breaks form conspicuous features in places, not only among the hills, but also in the low country, a marked example extending from Clifden to Cleggan Bay, this being in places over 100 feet deep and not more than thirty feet wide " (*Valleys, Fissures,* etc., pp. 97, 98). " If valleys are not connected with breaks in the underlying rocks, how is it that they occur in regular systems over large tracts of country ? Examine any, but especially a contoured map of Ireland, and it will be seen that the outlines—river valleys, lake basins and bays—occur in systems the general bearing of which may be indicated by lines. If such systems are not caused by breaks in the subjacent rocks, they must be due to chance—an alternative that even the most sceptical among the subaerialists could scarcely insist on. From the map of Ireland it will be learned that the most conspicuous system is an east and west one, that forms nearly parallel features ; two lines stretching across the island, respectively from Galway to Dublin Bay and from Clare to Dundalk

Bay; while to the north are other nearly parallel breaks that only extend eastward as far as the nearly north and south valley in which Lough Neagh is situated; while to the southward a break runs along the valley of the lower Shannon and from that to the nearly north and south valley marked by the Barrows. It can be traced as far as Arklow, but the eastern extension does not form a conspicuous feature, Further south, from Dingle Bay to Dungarvan, is another valley, that of the Blackwater, along which, as far east as Mallow, a large fault is conspicuous. Between these systems there are others which run in more or less parallel lines. In some these lines having an easting from north, in others a westing, while between these may be other systems extending more or less east and west. In a few places, especially if the rocks are ancient and metamorphosed, as to the southeast in Wicklow and Wexford, to the north-west in Galway, Mayo and Donegal, and to the north-east, are limited tracts having minor systems peculiar to themselves. Moreover, the principal lines in the last named extend into the south-west part of Scotland. The connection between the joints, breaks, valleys and lake basins of West Galway Kinahan shows elsewhere. He continues : " North of the Blackwater valley a fault has been proved ; in the valley that extends from Dingle Bay to Dungarvan Harbour ten large faults or systems of faults have been proved and traced ; all of which are connected with more or less marked features in the ground and have a general parallelism to one another ". These he enumerates and describes. " In Slieve Aughta there is only one large north and south valley, that of Loch Graney, but the east and west valleys are numerous, and lines of fault have been proved to occur in each. . . . The Scarriff valley to the south of the one last named has one accompanying fault, which bounds it on the north and seems to run from Feakle Lower to Mount Shannon ; on the coast it seems to extend to Loch Derg." Kinahan enumerates seven other faults, all connected with Slieve Aughta, running through different valleys, which he describes in some detail ; and he adds : " It may also be pointed out that the general bearings of the lines of coast are all more or less connected with the systems of lines in the adjoining portions of the

island, as if the lines that induced the valleys also had an influence on the coast features " (Kinahan, *Valleys, Fissures,* etc., pp. 99-107).

Kinahan also says how in tortuous valleys it is sometimes difficult to connect them with their associated breaks; nevertheless, after a little examination, it is seldom that proof cannot be found that all the different lines, no matter how irregular, have connections with breaks, either faults or joints. A ravine which illustrates this fact occurs on the eastern slope of Slieve Gallion, County Londonderry. This glen is most irregular, and after a casual examination its excavation might be supposed to be due solely to the influence of rain and rivers. This, however, is not the case, as the rocks are traversed by systems of joints—one system parallel to the strike of the beds, while the other is slightly oblique to it (*op. cit.*, pp. 90, 91).

Having described a large number of valleys due to fracture within our four seas, I will merely enumerate a few examples from other localities, and especially examples of transverse valleys. Brongniart mentions that many rivers, instead of following their natural valleys, cut across mountains. Thus the Rhine, after passing Lake Constance, cuts the chain running from north-east to south-west, and which forms on its left bank the extremity of the Jura and on the other side the beginning of the mountains of the Black Forest. It crosses this chain almost at right angles, in a rough channel and with impetuous course. From Bâle to Mayence it follows the natural valley of Alsace. Below Mayence it again traverses a mountain chain. On one side the chain is called the Eifel and forms the end of the Ardennes, and on the other the Westerwald. It crosses it not by a valley, but by a gorge from Bingen to Coblentz.

The Rhône cuts the Jura at its other end by a ragged channel with waterfalls, etc. It cuts the Jura at two points —at the fort of L'Ecluse and at Saint-Genix.

The Elbe, after crossing Bohemia, cuts the Saxon mountains which border it on the north by a defile from Theresienstadt to Pirna.

The Danube, after crossing large plains and travelling round mountains, cuts the southern Carpathians at Orsova.

The Yenissei, the Ob and the Irtish, after springing in lakes in Central Asia, cut through the mountains in the north. This is specially seen in the Yenissei when it leaves Lake Baikal.

The Ganges springs in a cavern and then cuts through a mountain.

In the Appalachians almost all the principal rivers cut through the chains and longitudinal valleys laterally. Thus the Great Kenhawn cuts the Laurel Mountains, a branch of this range, before it reaches the Ohio.

The Tennessee cuts the south-western extremity of the same chain at Great Lookout Mountain.

The Hudson cuts the Alleghanies at the highlands.

The Delaware cuts the Blue Mountains at their northern extremity.

The Susquehanna cuts the same range in the centre.

The Potomac does the same.

The Janero cuts four similar chains, with also the north and the south mountains.

The Orinoco at San Fernando de Atabapo turns suddenly to the north, pierces a chain of mountains, and forms the great cataracts of Maypurés and Aturés. Its waters for some distance seem divided into a number of basins by natural dykes.

The Maranon, a principal feeder of the Amazon, follows a longitudinal valley parallel with the Andes for some distance, but at San Juan de Bracamoros turns to the east, cuts through the mountains, and then cuts other outliers of the Andes by very narrow passages and leaves the defile at Pongo de Mauseriche.

The various rivers coming down from the mountains called Nieuwveld and Bambus Bergen cut through the primordial chains of Graaff Reinet and Lange Kloof (in South Africa).

These rivers have gentle currents and gentle beds everywhere except when they cut through the chains (*Dictionnaire des Sciences Naturelles*, xiv., pp. 35-37).

I have no doubt whatever that all these river valleys are connected with dislocations, as similar ones have been shown to be in better explored districts.

The most important and interesting transverse valleys,

however, from the point of view of our present discussion, are the transverse valleys called fiords, mostly cut in hard crystalline rocks and having straight-up sides, a ramifying series of smaller fiords at their heads and a curious contour in their floor levels, the foot of the fiord rising rapidly like the bow of a boat. We have shown how impossible it is to attribute these fiords to erosion of any kind, either aqueous or glacial. They are, perhaps, the most typical of all transverse valleys, and are found cutting at right angles at intervals into long ranges like the Dovre Fjeld and the mountains of Greenland.

By the theory of fracture caused by tension we have been discussing they become quite easy to explain, as do their perpendicular sides with their precipitous drops of several thousand feet. They occur, too, with singular uniformity, not only in high latitudes, as Croll and Geikie and others have argued, but in low latitudes, such as Dalmatia, Asia Minor and Provence, and in the tropics, as in Cuba, in the Congo district in Africa, in Equatorial America, etc. In all such cases it would seem they are directly due to the necessary breaks at right angles to the line of upward thrust which Hopkins has discussed. They are merely partially submerged transverse valleys, and, as Suess argues, they represent exactly the valleys of the Southern Alps in which the Italian lakes lie. If they were somewhat raised up most of them would form lake basins or have lake basins in their hollows. I will not enlarge on the fiords of Greenland and Norway just now, as we shall have to return to them later, and will only lay it down as apparently an indisputable conclusion that they are transverse fissures and nothing more. I will, however, say something of the Scotch fiords and the lakes of Western Scotland, which are merely raised-up fiords, and which form a good touchstone of our case; and I will quote from Mr. Kinahan, who has admirably stated the view to which I am devoted.

" An examination of the chart of Loch Fyne," says Kinahan, " is not without interest. The south portion of this fiord, as far north as Loch Gilp, bears nearly north and south, but the rest of it has a general bearing of north-east and south-west, the fiord widening out considerably where these two portions join into one another. When it is examined in detail, however,

other peculiarities will be observed. At the entrance of the bay and south of a line (north 70° east) drawn through Loch Tarbet, the deeps run about north 15° west, but at this line they are shifted considerably towards the west, while north of it they bear nearly north and south to High Rock, which lies due north-east of Maol Dubh Point. A little south-east of Maol Dubh Point there is a space over eighty fathoms deep, with one portion over ninety fathoms, and from this deep there extend others of less magnitude towards north 70° west into Loch Gilp and north 15° east to the Otter Spit Narrows, the ninety-fathom hole being at the junction of these two lines. . . . Immediately north of the Otter Spit Narrows the line of deeps is shifted considerably from the west towards the east, while north of this line the deeps bear north 20° east till they meet the break (north 20° west) coming down Gair Loch, at the junction with which there is a deep hole, over thirty fathoms deep, the surrounding bottom being less in depth. Also starting from this point is another line of deeps which bears north 40° east and extends to the Minard Narrows. At the Minard Narrows there is a complication, there being various small islands and only one narrow channel, which reaches twenty fathoms in depth. North of these islands the line of deeps is slightly shifted towards the west, and it bears about north 30° east as far north-east as Furnace, when its bearing changes to north 70° east; but in a short time, when Strachur Bay is nearly reached, it changes to north 25° east, and this line of deeps extends past Inverary into Loch Shira. From a little east of Furnace to Inverary the fiord is very deep, most of it being over fifty fathoms, while there are long holes in it over seventy fathoms deep. From Inverary towards the north 60° east the deeps extend for some miles, while the channel of the fiord at the north-east extremity bears nearly due north-east.

"From the summary just given," says Kinahan, "it is evident the fiord of Loch Fyne consists of a series of features which are more or less systematic, for the deep portions extend along regular lines; and where these lines cross or join one another there are extra deep soundings, and each shift in these lines corresponds with lines of features in the adjoining country. Glenshira is connected with a break in the

strata and Glenary appears as if it followed a similar line. . . . The sudden deep opposite Inverary lies at the junction of these two lines with those of the breaks that exist higher up and lower down in the main channel of the fiord, while a little further south-west one of the deepest spots in the upper portion of the loch is where the break occupied by the Douglas Water joins into and slightly deflects the main break. The island and shoals at Minard Narrows seem to be brought up by a fault, so also do the Otter Spit and the various islands and shoals in the bay " (Kinahan, *Valleys, Fissures*, etc., pp. 219-221).

" In the highlands of Scotland," again say Kinahan and Warren (two very experienced judges of such matters, and both members of the Irish Survey), " as far as we visited them we did not meet with a valley, ravine or lake basin unconnected with a break. . . . Loch Lomond occupies a remarkable valley, which has a general bearing of north and south corresponding with the lay of other important breaks in that part of Scotland. Extending from this valley eastwards and westwards are transverse features, some forming valleys, while others constitute greater or less depressions in the hills. That all these valleys and depressions are connected with dislocations in the underlying strata is manifest by the strike of the rocks being deflected, or the beds of rock rising to the upthrow of the faults, or by the shifting of conspicuous beds of rock. Loch Lomond must at no very remote period have been a fiord connected with the valley of the Clyde. . . . The lake is divided into two portions by the chain of islands stretching westwards from Balmaha. From the chart we learn that south of these islands it is shallow, rarely exceeding twelve fathoms in depth ; however, in a few places it is thirteen fathoms, and in one spot, east of Inch Murrin, a hole fourteen fathoms deep is recorded. North of the islands the lake gradually deepens to Ross Point ; it then shallows for a short distance, being only four to six fathoms deep on Hunter's Bank, which lies a little north-east of the *niver* of Douglas Water ; but on the north of this bank it immediately deepens to over twenty-five fathoms, and northward it gradually gets deeper and deeper till it attains the maximum depth of 105 fathoms due west of the hamlet of Culness. North of this place a deep portion

(over 100 fathoms) extends for nearly a mile, after which the basin gradually shallows to Whitepoint, between which and the end of the lake there is another deep portion, about thirty-four fathoms. . . . The features of the adjoining country and the shape of the bottom of the lake basin have a connection between them. The irregular deep east of Ben Dhubb occurs where the nearly north and south breaks and the north 30° east breaks meet or cross one another. The sudden deepening north of Hunter's Bank seems due in part to a fault with a downthrow to the north coming into the main valley from the east.

" The shallow to the south of the deepest spot is evidently on the north-west side of the fault line which is associated with the Tarbet valley. This appears to be a very recent fault, and it has considerably shifted the main fault of the valley. The Inversnaid and Inverglass faults also slightly shift the main fault ; while the deepest spot in the basin (105 fathoms) seems to be at the junction of the main and Culness valley faults." Kinahan concludes from these facts that we have a right to assume that the form of the lake basin is more or less connected with the breaks in the underlying strata, and he postulates a great north and south break, with transverse breaks and their accompanying fissures branching from it, afterwards widened and altered by meteoric abrasion. He further concludes that some of the dislocations connected with the lake are post-glacial, and that the features of the country both near Loch Lomond and Loch Katrine seem to indicate very recent movements and breaks in the underlying rocks (*Valleys, Fissures*, etc., pp. 210-216).

So much for fiords, either submerged or subaerial. I now propose to say something of the peculiar valleys cutting through mountain chains irrespective of the nature of the rocks, and which are known as cañons, of which such magnificent specimens exist in Colorado and other parts of America. These, as we have seen in the last chapter, have been attributed to the erosive action of subaerial agencies by the credulous and all-believing champions of uniformity.

It seems to me that it is not easy to discover examples of the effects of surfaces cracking under tension more marked

than those famous rifts, and less like any channels made by river or subaerial action.

They occur always in districts where there has been much disturbance; where a plateau or a vast dome of rocks has been lifted up, and where it is a necessary consequence that cracks should have occurred. Those who will not hear of their having been caused by such agencies always revert to the parallelism of the strata on each side of the gaps as being in some way or other an argument against their being cracks; but, as we have seen, this argument is, in fact, a very strong one in favour of the view here championed. This view is very largely accepted by Prof. Prestwich and by others, and is very largely confirmed by the actual observed results in certain instances of wide-spread earthquake action, as in the case of the famous earthquakes in Calabria at the end of the last century.

Dolomieu in describing that earthquake says : " The most common effect, of which a number of examples is seen in the territories of Oppide and Santa Cristina and on the banks of deep valleys or gorges in which run the rivers Niardi, Birbo and Tricucio, is where the inferior base having given way the upper grounds have fallen perpendicularly and successively in great trenches or parallel bands, each assuming its respective position, so as to resemble the benches of an amphitheatre; the lowest bench or terrace is sometimes 400 feet below its first position. This among others is the case of a vineyard situated on the border of the river Tricucio, near a new formed lake. It is in this manner divided into four parts, which hang in terraces one above the other ; the lowest part of the terrace fell from a height of 400 feet." This is surely very eloquent for our purpose.

It seems to me again that we cannot look at the photographs and pictures of the surface of the country through which the cañons run without being impressed with the fact that these fissures running in all directions, with perfectly sharp lips, resemble only a great mass of rock which has been split and fissured by some gigantic earth shock. It has all the appearance of the kind of fractures we get when a solid surface, on a smaller scale, has been smashed and shivered by some blow or impact, and the water which flows in the fissures flows

there not because it made them and is still making them, but because it found them ready made.

The work done by Newberry in the Colorado country was worthy of the reputation which he has acquired. So far as the observation and description of facts is concerned it was admirable and admirably published. In regard, however, to his inferences and deductions I am by no means of the same opinion. No doubt they came very opportunely to the aid of the dominant school of uniformitarians, and it is not wonderful that, like a parrots' chorus, they have iterated and reiterated the conclusion which he arrived at, that the great cañons with their perpendicular sides are the result of river erosion and not of subterranean movements. I should like to quote an equally distinguished geologist on the same subject. Dr. J. W. Foster, in his monograph on the Mississippi valley, says : " The great cañons of Colorado, forming gorges from 3,000 to 6,000 feet in depth, amid whose intricacies the traveller is liable to become almost hopelessly involved, are regarded by Newberry as belonging to a vast system of erosion and wholly due to the action of water. On the other hand, when we see, for instance, along the rock-bound coast of Lake Superior, upon which the waves have dashed for thousands of years, the most delicate etchings on the rocks perfectly preserved, we confess that in running water we fail to recognise an adequate cause to account for the excavation of these profound gorges, and although the geologist, like the actor, should ever have in mind the advice of Horace, *Nec Deus intersit, nisi dignus vindice nodus*, yet here is an instance in which we think the fire god may be properly invoked, and to his interposition these tremendous events may be in part ascribed, or, in other words, that the form and outline of these chasms were first determined by plutonic agency. Every explorer describes the heavy accumulations of volcanic matter which cover the most superficial materials in the elevated regions between the Rio Grande and the Colorado Desert, and in fact throughout the entire range of the Rocky Mountains. It would seem that while aqueous causes were in full activity over all these regions, igneous causes exhibited an activity equally conspicuous " (*The Mississippi Valley*, pp. 339, 340).

Prof. Prestwich, who is a champion of river erosion of

valleys, in writing on this subject in his *Geology*, says of the Colorado cañons that they present a yet unsolved problem. "The majority of American geologists," he says, "it is true, ascribe these deep and almost inaccessible gorges entirely to water erosion, but still some of them allude to the possibility of the direction having originally been given by fissures on the surface. The evidence, to my mind, does not seem altogether conclusive. . . . That they are not lines of fault has been sufficiently proved, inasmuch as the strata on either side of the cañons are on the same level. Further than this, the cañons traverse the plateaux irrespective of faults, folds and of monoclinal escarpments, passing across the one and through the others without turning to the right or left." Prof. Prestwich inclines to the view which will assuredly prevail presently, that when the land was elevated the surface was rent and fissured to great depths, these fissures forming, from the first, lines of drainage which have never varied. As he says, it is not necessary that a line of fissure should be a line of fault, and must not be confounded with it. "The physical conditions of this great area are peculiar and abnormal. It forms not a ridge or an isolated high plateau; but a vast dome-shaped mass rising in the centre to the height of 12,000 or 14,000 feet. This form of surface may have produced lines of rent without faulting, or it may have so opened the joints of the rocks as to have produced a labyrinth."

Writing of Labrador, Mr. Steinhauer says: "In several parts of the country the rocks are intersected by chasms running generally in a right line to a considerable distance, as if intended for the receptacles of future rains. . . . The narrow passages which divide the coast into numberless islands almost seem to be similar chasms occupied by the sea, few, if any, of these islands being alluvial, but high barren rocks" (p. 490).

The most striking, perhaps, and eloquent of all the facts known to me in this behalf are the famous Zambesi Falls, discovered by Livingstone. These falls are called by the natives Maswatunya (smoke does sound there), or more anciently Shongwa. He thus describes them: "Creeping with awe to the verge I peered down into a large rent which had been made from bank to bank of the broad Zambesi, and saw

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that a stream of 1,000 yards broad leaped down 100 feet and
then became suddenly compressed into a space of fifteen or
twenty yards. The entire falls are simply a crack made in a
hard basaltic rock from the right to the left bank of the
Zambesi, and then prolonged from the left bank away through
thirty or forty miles of hills. . . . I judged the distance which
the water falls to be about 100 feet. The walls of this gigantic
crack are perpendicular and composed of an homogeneous
mass of rock. The edge of that side over which the water
falls is worn off two or three feet, and pieces have fallen away,
so as to give it somewhat of a serrated appearance. That
over which the water does not fall is quite straight, except at
the left corner, where a rent appears and a piece seems inclined
to fall off. Upon the whole, it is nearly in the state in which
it was left at the period of its formation. The rock is dark
brown in colour, except about ten feet from the bottom, which
is discoloured by the annual rise of the water to that or a
greater height. . . . The fissure is said by the Makololo to be
very much deeper farther to the eastwards. . . . If we take the
want of much wear on the lip of hard basaltic rock as of any
value, the period when this rock was riven is not geologically
very remote " (*Missionary Travels*, pp. 518-523).

Livingstone argues that the whole country hereabouts once
formed a gigantic lake, which stretched from 17° to 21° south
latitude. The whole of this space, he says, is paved with a
bed of tufa more or less soft. Wherever ant-eaters make holes
in it they throw out fresh-water shells like those of Lake
Ngami and the Zambesi. The Barotse valley was another
lake of a similar nature, and one existed beyond Masiko and
a fourth near the Orange river. " The whole' of these lakes
were let out by means of cracks or fissures made in the
subtending sides by the upheaval of the country. The fissure
made at the Victoria Falls let out the water of this great
valley, and left a small patch in what was probably its deepest
portion, and is now called Lake Ngami. The Falls of Gonya
furnished an outlet to the lake of the Barotse valley, and so
of the other great lakes of remote times. The Congo also
finds its way through a narrow fissure, and so does the Orange
river in the west, while other rents made in the eastern ridge,
as the Victoria Falls, and those to the east of Tanganyika,

allowed the central waters to drain eastwards. All the African lakes hitherto discovered are shallow, in consequence of being the mere *residua* of very much larger ancient bodies of water. . . . Deep fissures were made probably by the elevation of the land, proofs of which are seen in modern shells embedded in marly tufa all round the coast-line" (*ibid.*, pp. 527, 528). Subsequently, after another visit in 1860, Livingstone wrote: "The depth down which the river falls without a break is not 100 feet, as was formerly conjectured, but 310 feet, and the breadth instead of 1,000 yards is between one statute and one geographical mile. The lips of the crack at Garden Island are probably more than eighty feet apart, for no one could throw a stone across. . . . The crack is prolonged in a wonderfully zig-zag manner. The promontories formed by the zig-zag fissure are of the same level as the bed of the river above the falls. These tops are so flat and narrow that a few paces enables one to see the whole river on each side of him 300 or 400 feet below jammed in a space of twenty or thirty yards. Like the ledge over which the river rolls at the falls, the sides of the promontories are nearly quite perpendicular, showing that the formation of the crack is of comparatively modern geological date. The river runs in the crack some thirty or forty miles. . . . The total descent made by the Zambesi between the Great Falls and Sinamane's, where it is smooth again, as found by the boiling point of water, is 1,600 feet."

Speaking of these falls, Mr. John Murray says: "The discovery of the Zambesi Falls would seem to have been reserved until the present time, in order to refute a leading tenet of modern geology, and to prove the utter impotence of water to cut through hard rock. The conclusion seems irresistible that the fissure was made for the river to pass through, possibly by some shrinkage of the basaltic rock when cooling down from an incandescent state" (*Scepticism in Geology*, pp. 68-70). This is assuredly good sense and good science.

One of the most remarkable features in the topography of Africa is what my friend Dr. Gregory described as the great rift valley in the delightful book he wrote about Mount Kenya. This valley is part of a great depression "which begins with

the Dead Sea, extends down the Red Sea, and ends at Tanganyika ". In describing the origin of this valley, as illustrated by his own observations, he says : " On emerging from the Kikuyu forests we entered one (*i.e.*, a valley) which was straight in direction and bounded by parallel and almost vertical sides. Its characteristic features were that its lines were straight and that its angles retained some of their original sharpness, for the direct action of faults and earth movements still dominated the scenery. An hour after entering this valley we reached the edge of the great rift valley, which, like the former, must be directly due to earth movements over the plateaux of Mau and Kikuyu, and were continuous across the site of the rift valley. A double series of north and south faults cut through the plateaux and allowed the block of material between them to subside. This left a great open rift valley, or, to use Prof. Suess's term, a *graben*. In this method of valley formation strips of country have fallen owing to a series of parallel cracks or faults, and thus a valley has been formed with precipitous and sometimes step-like sides. Such valleys have long been known in America, and the extraordinary steepness of their bounding walls may be seen in photographs of the Yosemite cañon in California " (Gregory, *The Great Rift Valley*, pp. 326, 327).

Conder writes : " The western shore of the Dead Sea is bounded by steep precipitous cliffs, at the foot of which are marls and conglomerates belonging to an ancient sea level. At the top of these cliffs are marls of a similar character, giving a second level ; and from these the marl hills rise rapidly to a third level—that of the Bukeya or raised plain, situate at the foot of the main chain of hills and below the Convent of Mar Sabon. This gives a series of three successive steps, each of which seems at some period to have formed the bed of a lake, under conditions similar to those of the present sea. There is, however, a very curious feature observable : the narrow valley running north and south and separating a line of chalk cliffs, immediately adjoining the Bukeya, from the hard dolomite beds of the main chain. It is, in fact, evidence of a fault or sudden fold in the strata, the existence of which seems to have been hitherto unsuspected. Advancing north we find a broad basin north of the Dead Sea

in which Jericho stands, which has an exact counterpart on the east side of the valley. The same contortion of strata is remarkable. . . . From this point we succeeded in tracing an ancient shore line at a level equal to the second step on the western shore for a distance of over twenty miles up the valley. Thence a narrow gorge, with strata less violently contorted, extends for some ten miles. The valley then broadens again. . . . I have submitted these observations to professional geologists, and their opinion confirms that which I formed on the spot, that the Jordan valley was caused by a sudden and probably violent depression in times subsequent to the late cretaceous period " (*Report of the Brit. Assoc.*, 1874).

Turning from Africa to Asia, Murchison says : " When the traveller passes from the valley of the Serebrianka to that of its recipient the Tchussovaya, still more is he struck with wonderment at the unquestionable evidences, amidst intensely dislocated rocks, of the ruptures by which the deep narrow chasm has been formed in hard crystalline rocks, in which a lazy stream flows, which, not descending from any altitude, has had no excavating power whatever, and, like our own meandering Wye, has flowed on through clefts in limestone during the whole historic and prehistoric period without deepening its bed. Rivers which are not torrential, and do not descend from heights, cannot possibly have produced, nor even have deepened, the natural hollows or chasms in which they flow " (*Journ. of the Royal Geog. Soc.*, xxxiv., ccxxii.).

Dr. C. M. Bell says, speaking of Mazanderan : " From the top of the range down to the alluvial plain there is little in the course of the two rivers, the Tatar and the Heraz, which can be attributed to the action of mere running water. Both ravines, especially that from Demavend to Amol, are continuous rents from the top to the bottom of the range, and, narrow though they be, it would be difficult to point out a single spot where a correspondence of the strata on the opposite sides of the ravine exists. They have evidently been shifted by the successive convulsions which have elevated the range " (*Geol. Trans.*, 2nd ser., v., 581).

Referring to Strickland's view that the mountains of

Kaptan Alan in Asia Minor had been cut through by the river Hermus, there is a note by the joint author of the paper, Mr. W. J. Hamilton, who says that in justice to himself he is obliged to state that he does not agree with the opinion that the cutting across the *coulée* and the formation of the cliffs are at all owing to the effect of running water acting upon the adamantine basalt. The very circumstance of the perpendicularity of the sides is an argument against it. He is rather inclined to attribute it to the fall of the basaltic masses in consequence of their having been undermined by the waters, the operation of which cause may have been hastened by fissures and crevices produced by the numerous earthquakes to which this country has at all times been exposed (*Geol. Trans.*, 2nd ser., vol. vi., p. 34, note).

A stupendous cañon, perhaps the most striking thing of the kind in the world, which separates the Taurus range from that of Amanus, is, according to my friend Sir Charles Wilson, coincident with a gigantic fault, the strata on either side of it being extremely dissimilar.

Speaking of the rents and ravines in the basaltic beds of Cutch, Grant says : " One of these ravines, near the village of Doonee, is fifty or sixty yards broad and nearly 100 feet deep, and its perpendicular sides are composed of compact columnar basalt of a greenish-grey colour, the columns being perfect polygons and of a very large size. This rent must have been formed by some convulsion, as it reaches nearly to the summit of the hill ; and the only water that could ever have flowed down it being that which falls on its sides and bed, and must be very little " (*Trans. of the Geol. Soc.*, v., p. 312).

Speaking of the hill of Nugia Soorud, near the village of Nukutrana, he says : " The hill is divided into two parts by a narrow tortuous cleft, the sides of which are nearly perpendicular and composed of irregularly triangular prisms of basalt. The cleft is not more than four feet wide at the bottom, though it is somewhat broader at the top, and as it passes completely through the hill we must suppose the whole to be similarly composed " (*ibid.*, p. 316).

The rivers Kistnah and Pennar, according to Voysey, pass through the Nulla Mulla range by gaps or fissures, " which," he says, " have been produced by some great convulsion,

which at the same time that it formed the beds of these rivers gave passage to the accumulated waters of some vast lakes situated near the outlets. . . . The tortuous course of the Kistnah is bounded for upwards of seventy miles by lofty and precipitous banks, which in some places rise 1,000 feet above its level, the opposite sides of the chasm corresponding in an exact manner. Ravines of this description are not infrequent all over the range, and the exact correspondence of their salient and re-entering angles, together with the abruptness of their origin, totally preclude the supposition of their being hollowed out by the action of running water " (*Asiatic Researches*, vol. xv., pp. 123, 124). Malcolmson says that such seems also to have been the case where the Pennar passes through a narrow gorge in the Gundicottah sandstone hills. Through the upper part of its course it flows over a flat country covered with alluvial soil at right angles to the hills, but it finds an exit through them by a fracture in the wall which in former times had apparently dammed up its waters (*Geol. Trans.*, 2nd ser., v., pp. 574, 575).

Coming nearer home, Mr. Murray, speaking of the two gorges of the Rhine at Bingen and the Danube at the Iron Gates, says : " No impartial spectator looking at these two defiles can deny that they have the appearance of clean fracture, effected *à un seul coup*. Their sides are flat and smooth, and where the beds of strata project they present sharp angles or splintery edges in distinction from curved surfaces. . . . The well-known gorge, the Via Mala, is so absolutely a crack through a mountain that the two sides, 1,500 feet high in places, are barely separated by two or three feet of interval. From the freshness of the fracture they seem to have been torn apart only yesterday and ready to close again at any moment " (*Scepticism in Geology*, pp. 64, 65).

Mr. Murray again says : " Geologists acquainted with the Alps need not to be reminded of such examples as the Glärnisch, where an entire mountain is rent from top to bottom in a precipice 6,000 feet high ; nor of the Galanda, torn from the opposite range of the Kuhfersten, both in eastern Switzerland. Such instances of the effects of energies now extinct may be multiplied a hundredfold in almost every part of the world."

Bone says of the streams of Transylvania that most of them run through the chains by gorges of very recent fracture (*Proceedings of the Geol. Soc.*, i., p. 243).

This will suffice to illustrate the various forms of clefts and fractures known as cañons, ravines, etc. A few words now about another form of valley.

De Lamétherie attributes the formation of valleys in many cases to what he calls *affaissements, i.e.*, subsidences, and quotes the subsidence of part of Scilla in 1783, and that which occurred at Sallanches in Savoie in 1751, described by Donati (*Théorie de la Terre*, v., pp. 148, 149).

M. G. de la Noe, in discussing the various origins of valleys, enlarges on the sinking of the ground between two faults, thus forming valleys flanked by solid terraces, each terrace marking the land between two faults. He says : " La vallée du Rhin entre Bâle et Mayence est depuis longtemps connue comme un exemple typique de ce genre de vallées ; la vallée de la Saône est probablement dans le même cas ; sur une petite échelle, nous citerons la vallée de l'Orche. En ce qui concerne en particulier la vallée du Rhin, nous ferons remarquer que ceux de ses affluents de la rive gauche qui descendent du massif des Vosges traversent précisément une série de failles disposées en escalier " (*op. cit.*, p. 155 ; see also Bleicher, *Essai de Géologie comparée des Pyrénées, du Plateau Central et des Vosges*, 1870, pts. iii., iv.).

Another kind of valleys which was discriminated and described by Buckland has been much neglected of late years—namely, what he and Scrope call valleys of elevation. A few paragraphs will suffice to describe them.

" At Kingsclere there is a sudden and unusual elevation of the chalk accompanied by fracture and an inverted dip. . . . At Inkpen Hill strata of chalk, higher than elsewhere in Britain, dip on either side of a central axis nearly north and south. This opposite dip is seen in the chalk pits in the valleys between Inkpen and Kingsclere, but from Highclere Park to Kingsclere more distinctly marked by the bursting up of beds of upper greensand or freestone from beneath and between two escarpments of chalk. These form the vale of Kingsclere " (Buckland, *Geol. Trans.*, 1st ser., ii., p. 121).

" This unusual position," he continues, " of a valley composed

of greensand within an apparent area of chalk derives illustration from similar instances occurring as we follow the edge of the chalk south-westward from Kingsclere through Wilts and Dorset towards Weymouth. These all have the same features of a valley circumscribed on all sides by an escarpment whose component strata dip outward from an anticlinal line, running along the central axis of the valley. The first of these is near the villages of Ham and Shalbourn, five miles west of the vale of Kingsclere. The escarpment here is continuous on every side but the south, its western end being separated from the vale of Pewsey by only a narrow and low bar of chalk stretching like a bridge across.

" Another similar valley occurs at Bower Chalk, ten miles east of Salisbury. Here also the strata consist of chalk lying on greensand. The isthmus of chalk which separates it from Alvedeston is known as Cleve Hill.

" A third such valley may be seen at Poxwell near Osmington, seven miles north-east of Weymouth, nearly elliptical in shape and not much more than two or three times the size of the Coliseum. Here the enclosing strata are Portland stone and Purbeck marble, but it otherwise agrees in that the strata dip outwards from a central axis and in the valley being surrounded by escarpments " (*Geol. Trans.*, 2nd ser., vol. ii., p. 122).

" The drainage of these valleys," says Buckland, " is generally effected by an aperture in one of their lateral escarpments and not at either extremity of their longer axis, as would have happened had they been simply excavated by the sweeping force of rapid water ; and it is utterly impossible to explain the origin of such valleys by denudation alone without referring the present position of their component strata to a force acting from below and elevating the strata along their central line of fracture. Similar enclosed valleys with similar axes or centres of elevation occur in the Bristol coal basin, where, in following the anticlinal lines that traverse that district, it is seen that the strata on either side of them are in some places elevated into lofty ridges and in others pass along the lowest points of the valleys. An example occurs in the vale of Westbury near Bristol, and also near Thornbury, Berkeley and Newnham on the Severn ; a third occurs in the vale of Ely west of Llandaff.

Several similar valleys consisting of basins of old red sandstone enclosed by hills of limestone occur along the great anticlinal line extending across the Severn from the western extremity of the Mendip Hills through Glamorganshire and Pembrokeshire to Milford Haven."

After referring to the case of the Wealden valley, he says: " The facts conspire to the conclusion that not only many enclosed valleys similar to that of Kingsclere, but also, in a less degree, many open valleys similar to that at Pewsey, and the great central valley of Kent and Sussex, though largely modified by denudation, owe their origin to an antecedent elevation and fracture of their component strata " (*Geol. Trans.*, 2nd ser., ii., p. 125).

In the eastern part of the Weymouth district we have examples of valleys of elevation on a small scale in the three little circus-shaped valleys of Morgnes Down, Poxwell and Sutton Poyntz. " All these three valleys are of the shape of a Roman circus, and if the basset edges of the strata were cut into benches the central area would be visible to persons seated on every part of them. All three valleys are situated in a straight line east and west, parallel with the grand axis of the Weymouth district, and also parallel to two great faults adjacent to them " (*Geol. Trans.*, 2nd ser., iv., pp. 33, 34).

The valleys of elevation just described lead us naturally to a similar phenomenon on a much larger scale, whose explanation has baffled so many inquirers. I have in previous chapters criticised the views of those who have attributed the Wealden valley to erosion, either subaerial, fluvial or marine, and now propose to explain why we should revert to an older view, namely, that it has been caused by disruption. I will first give a short description of the Weald area in the graphic words of Mr. Kinahan. He says: " The Weald is bounded on the north, west and south by chalk hills or downs, while near its central line is an axis of hills, which in places is higher than the average height of the downs. These central hills are of rocks the oldest in the area; consequently they are hills of elevation. . . . If on the rocks forming these hills the absent strata were replaced, the hills would be between 2,500 and 3,000 feet in altitude, or, in round numbers, about 2,000 feet higher than at present. The North Downs are on

an average 700 feet high and the South Downs 800 feet; consequently the central hills would be 1,300 and 1,200 feet higher than the present North and South Downs respectively. These are about eighteen and twelve miles from the central axis, and there are dips northward and southward (ignoring all minor flexures and rolls) of about seventy-eight feet and 100 feet to a mile, equal respectively to about 1 in 68 and 1 in 53, or about an angle of one degree both ways " (*Valleys, Fissures*, etc., p. 196).

The features just described are clearly only consistent with some subterranean movements. If the chalk was once continuous in the form of a horizontal mass, "the North and South Downs must have sunk and the Hastings sands must have been shoved up". No erosive action could have caused the present position of the Hastings beds. The arch-like contour of the chalk beds of the Downs show, however, that they were raised as the central ridge. What, then, would be the result of a movement of upheaval possibly of the chalk in the fashion supposed along the lines of the Downs? I will again quote Kinahan, who has stated the case well. " When a sheet of rock is bent into anticlinal and synclinal curves it may form unbroken arches, or the strata may gape in places, and form open fissures along lines of weakness, the gaps being equal to the radii minus the cosines of the angles of the slopes. This in the case of the Weald, if we ignore the minor flexures and consider it one anticlinal, would be eighteen miles multiplied by 1, minus the cosine of 1° + 12 miles multiplied by 1 minus the cosine of 1°, which would give a valley only twenty-five feet wide from escarpment to escarpment if the beds opened together, which, however, is improbable, the different groups of rocks being made up of such dissimilar materials. In such a case each bed would have retained its relative position to its fellows ; but if the beds were capable of bending without breaking, and would not stretch, the original position of each would have to change, as each bed must move on the one below it up to the axis of each anticlinal curve. If, however, the upper beds were ruptured while the lower beds were not, it is not unreasonable to suppose, especially if there was an upward as well as a horizontal thrust, that the upper beds might remain fixed at their unbroken ends, while the lower

beds would be pushed along below them and up into the
break, such a movement naturally taking place along a soft
weak bed. Thus, if the chalk in the Weald was broken, but
fixed at one side, the greensand would be pushed out from
under it along the gault; and the Hastings sand beds might
be pushed out from under all along one of the Weald clays.
Such a movement in the Weald, although the average angle
of dip were only one degree (it is really higher), would form
terraces between the outcrops of the chalk and greensand,
and the greensand and the Hastings beds in the north and
south parts of the valley respectively, fifteen feet and ten feet
wide " (Kinahan, *Valleys, Fissures*, etc., pp. 201, 202).

It seems to me that this is a perfectly rational explanation
of the mysteries of the Weald. It is in essence not new. It
was the view maintained by Scrope and other old masters
until the so-called uniformitarians came in and swept away
their inductions by reviving the arbitrary hypotheses of
Hutton and Playfair. Thus Scrope, writing in 1825, says : " A
longitudinal crack opened across the beds parallel to the axis
of elevation. The chalk sinking on beds of clayey marl
slipped away on either side from the axis, leaving bare the
lower strata of greensand." Again : " The partial subsidence
of this formation upon the slippery beds of the Weald clay
disclosed in turn the iron-sand which forms the visible axis
of this ridge " (Scrope, *Considerations on Volcanoes*, x., p. 213).

Martin says that " simple *transverse fissures* are not the only
appearance of displacement exhibited by the chalk strata.
They have suffered other displacements due to the material
on which they rest. " If the fissile character of the stony
strata determined their division in straight and broad lines,
the Wealden formation would be torn and contorted in a
widely different way. For a stratum of clay of great thick-
ness carrying stone barely sufficient to give it stability would
tear rather than split in the act of displacement, and such a
divergence of fissures as might be expected in so tenacious a
mass can be readily traced in every part of the Weald surface.
This divergence and laceration has therefore modified the
disposition of the stony strata still superincumbent upon the
clay, and the subsidence, elevation and contortion consequent
thereon are everywhere visible in the dip and variable bearing

of all their masses. In appreciating the evidence of these acts of laceration and fracture it must not be forgotten also that strata of various structure, far below these under review, have suffered the same disruption, and by the variable nature of their fracturings have operated to modify and in some cases to obscure a great part of the direct testimony of the order here described. The pressure of superincumbent strata of great thickness must also be taken into account " (*ibid.*, pp. 61, 62).

Mr. Murray says : " From the evidence afforded to us by numberless sections of disturbed strata it appears that the outer folds of bent rocks have snapped asunder under the strain of severe pressure long before assuming the shape of complete arches. Thus the amount of chalk destroyed by denudation in the Weald may have been comparatively small when we allow for shrinkage and fracture ensuing soon after the pressure was applied. To use a homely comparison, the chalk escarpments may have parted asunder like the sinews in a shoulder of mutton on the application of a knife " (*Scepticism in Geology*, pp. 44, 45).

Similar slides of upper beds over the lower are frequent enough when the upper ones are either harder or softer than the lower ones, as in the case of sliding bogs and masses of gravel and sand on slopes and many ruined undercliffs. To me it seems that the only suggestion yet made in explanation of the Weald phenomena which meets all the difficulties is the one I have here set out, namely, that it is a case of a longitudinal crack and fissure caused in the chalk by the upheaval of the North and South Downs, and of the subsequent or contemporaneous forcing up through the fissure of the nether beds forming the central axis, which forcing up has made the fissure to gape out to a great extent, thus causing its two flanking walls to become the escarpments which face each other at several miles apart, the chalk being dragged over the softer beds below it. This old and sound view seems to me to account for the facts in a way which no other theory does. It accounts for the laceration of the rough and rugged surface of the Weald area. It accounts for the absence of flints and other débris of the chalk which assuredly would have been there if the chalk

had been denuded in the way usually described. It accounts for the escarpments, for the peculiar shape of the Weald area, and I know nothing against it except the fanatical opposition of a quite modern school of geology to anything in the shape of violent or rapid change. It seems, further, to me that the same explanation that I have here applied to the Weald must also be applied to the disappearance of the chalk, etc., from the area round the Wash, caused by the uplift of the chalk downs of Lincolnshire and Norfolk. A similar explanation should also be applied to the valleys of elevation above referred to, where the surface beds have apparently been scooped out, but which are, in fact, instances of the tenacious chalk or other surface beds having been dragged off these districts when the chalk was uplifted, dragged asunder and thrown into its present folds.

Let us now turn to the concluding portion of this chapter and discuss the origin of those lakes and tarns which are true rock basins, that is to say, those which have a rim and edge of solid live rock all round their contour and are not merely reservoirs dammed back by débris of different kinds. The researches of Marr, Watts and others have in recent years shown that a great proportion of the mountain tarns which have been hitherto mistaken for rock basins are, in fact, nothing of the kind, but have somewhere round their margins a gap which has been filled up with moraine or other débris, and that is often the case even when the draining stream is seen to issue from the lake and to flow over live rock, the undisclosed dam being elsewhere.

With all such reservoir lakes we have nothing at present to do. What we have to explain are the true rock basins. As we have seen, they are not, so far as we can judge from the evidence, the result of erosion either by ice or water, and if not they must be the result of deformation of the ground either by way of subsidence or by the bending and twisting of the beds of rock so as to make cups or saucers of various shapes and sizes.

A considerable number of lakes exist in hollows which seem unquestionably to have arisen from a subsidence of ground, a fact which has been very much neglected by the champions of uniformity. Thus Bakewell tells us how the volcano of the Pic in the island of Timor, one of the

Moluccas, is known to have served as a prodigious watch-light, which was seen at sea at the distance of 300 miles. In the year 1638 the mountain during a violent eruption entirely disappeared, and in its place there is now a lake. Many of the circular lakes in the south of Italy are supposed to have been formed by the sinking down of volcanoes.

In regard to lakes formed by deformation of the ground, I will quote some opinions of experienced explorers in the field on the subject, and begin with my friend Prof. Bonney.

" Those geologists," he says, " who are unable to accept Sir A. Ramsay's hypothesis attribute the larger lake basins to differential movements of the earth's crust. A mountain range or chain, they say, consists primarily of a group of parallel folds in this crust. Suppose such a region to have been sculptured by the usual agencies into hills and valleys ; the latter, as carrying off the drainage, will run more or less athwart the lines of folding. Suppose next, that, at a very late epoch in the history of the chain, a new set of earth movements be initiated along the old lines, though not necessarily with the same intensity or effect in every part. The result will be that the floor of a valley will here rise above and there sink below its former level : the line which once shelved gently outwards will be bent into a curve ; the ascending part will dam up the river ; the descending part (higher up the valley) will form a basin in which a lake will accumulate " (*Ice-Work, Present and Past*, p. 90).

" If the lateral pressures," again says Bonney, " by which a mountain chain has been formed have begun again to act after an epoch of comparative rest, during which the folded masses have been carved into peaks and valleys, it is more probable that alternating zones parallel with the axis of the chain would be affected by uplifting and down-sinking movements than that the *massif* would rise and sink uniformly as a whole. Probably, if such differential movements were comparatively slight, they would be more marked towards the outer part of the chain nearest to the region on which incoherent materials had been recently deposited. Suppose, then, the outermost zone to rise and the next within it to sink, that part of the river valley would at once be converted into a lake. As a simple illustration take two points, A and

C, in a valley twenty miles apart, and B half way between them, and suppose the fall to be ten feet a mile; B is 100 feet above A, and C the same height above B. Suppose C to remain fixed, B to sink 400 feet, A to rise 200 feet, *i.e.*, to the level of C. A basin is now formed twenty miles long, which at its middle point under B is 500 feet deep " (*Geol. Journ.*, vol. i., pp. 495, 496).

" The advocates of this hypothesis cite, in confirmation of it, the region of the Great Lakes in North America. These lakes . . . appear to be true rock basins. They vary in shape, and the forms of some, if only their scale were reduced, would find parallels among the Alpine lakes; indeed, at one time there was a disposition to claim them as the work of ice. But by Professor J. W. Spencer, and other geologists of Canada and the United States, these lakes are shown to be part of a great river system. . . . Not only were the Great Lakes produced by differential movements of the earth's crust, after its leading outlines of hills and valleys had been sculptured, but also these movements have been subsequently continued " (*Ice-Work, Present and Past*, pp. 90-92). Again he says: " Lakes may have been formed by a subsidence, not local, but general, affecting a considerable district parallel with the average trend of the mountain range. . . . Subalpine lakes were portions of valleys. . . . At a time geologically recent the same forces as had produced the mountain ranges, by wrinkling and doubling into parallel folds a portion of the earth's crust, again operated, developing a comparatively slight flexure which affected the level of the floors of the valleys. These, at one place, may have been slightly pushed up; further back they may have curved gently downwards, bending the sloping floor into a hollow, in which water would gradually accumulate as the subsidence progressed. An examination of a geological map of the Alps indicates that the majority of the lakes can be grouped in zones connected with the trend of the ranges, like Orta, Maggiore, Lugano, Como, Iseo, etc.; and if the question be asked, Why, if this be the true explanation, has not every Alpine valley a lake near its mouth, the answer may be returned that the subsidence was not necessarily uniform, and that, as it is, where a lake is wanting a stony plain usually occurs to mark where one formerly existed. . . .

Those lakes which were most shallow would be filled up by débris." Bonney further adds, in support of the view that such lakes as Constance, Lucerne, Zürich and Geneva are not due to erosion, that the subaqueous contours of their basins, instead of being smooth and featureless, are irregular like subaerial contours (*Story of our Planet*, pp. 153, 154).

Views like these were held by the older masters. Thus Bakewell has some shrewd remarks on the origin of certain lake basins. He says: "The steep escarpments which the calcareous mountains of Switzerland and Savoy present on one side of the lakes which they border indicate that the beds of the lakes were formed in the hollows that had been left by the elevation of the mountains. The beds of the mountains on the side opposite to the escarpments generally slope down to the lakes; hence M. de Luc inferred that it was these mountains that had sunk down and left the chasm which forms the bed of the lake. Indeed, it is highly probable that when the beds of rock were broken and elevated in one part the beds adjoining would sink down leaving vast chasms, which were soon filled with water, and formed lakes. It seems quite certain that the lakes in the valleys of mountainous countries could never have been excavated by the rivers that flow into them. The great lakes of North America are situated upon a vast extent of tableland about 800 feet above the sea, but the country is so level that the rivers which flow into the lakes, and those which empty themselves in the Gulf of Mexico, are only separated at their sources by elevations not exceeding a few feet, and when swelled by rain the northern and southern rivers sometimes interlock. In this plain there are no mountains. These lakes were probably formed by partial subsidences at the epoch when the whole country was upheaved from the ocean" (*Introduction to Geology*, pp. 585, 586).

"It would appear impossible," says Hopkins, "not to ascribe the origin of the lakes of Coniston and Windermere to the dislocations with which they are so immediately associated; nor does it, indeed, seem possible to account for the existence of any of the larger lakes independently of similar dislocations. Taking Wastwater, for instance, its depth is found to be about forty-five fathoms, so that its bottom is

probably almost a hundred feet lower than the surface of the sea. . . . The lake could only be formed by a relative subsidence in its bottom. If this relative subsidence does not extend to the mouth of the valley or be less there than in the upper end of it, a lake will necessarily be formed. It is probable that in some of the English lakes the extension of the subsidence towards the mouth of the valley has been arrested suddenly by a fault transverse to the valley, as appears from the great depth in the case of Wastwater at an inconsiderable distance from its lower extremity. This general explanation will apply to all the lakes of the district, and appears to me to be the only intelligible one which can be given of their origin."

"It would be absurd," continues Hopkins, "to suppose that the ranges of the faults in these valleys are confined to those spaces only where we now find demonstrative evidence of their existence. There can be no doubt of their extension frequently along the whole course of such valleys, or in other cases beyond such limits" (*ibid.*, pp. 75, 76).

Kinahan again argues that, unaided by cracks, fissures and faults formed during the movements in the earth's crust, ice is incapable of eroding out rock basins, and he especially refers to the issue as tested by a great number of Irish basins in Connaught. "The rocks of that district," he says, "have suffered at successive periods from the disturbances due to the movements in the earth's crust, as they are not only folded and contorted, but also faulted and displaced to a very remarkable extent. Some of the faults are very ancient . . . while others are post-glacial. The rock basins in Connaught may be in corries (so called from the Celtic *coira*, a pot or cauldron), *i.e.*, in bowl-shaped valleys, or situated in maums (from *madhm*, the inside or hollow of the hand), *i.e.*, cols or mountain gaps and passes, while others are on level plains. A connection between them and one or two lines of master joints or faults is always apparent, which suggests that the latter must have materially helped in the formation of the basins. . . . All the stretches in the basins coincide with lines of breaks, faults or other shrinkage fissures, as well as all the transverse guts and bays. A good example is the very irregular one called Lough Conga. . . . All the lake basins

in this country widen or contract in accordance with the number of faults or joint lines which meet or cross in the area occupied by them" (*Valleys, Fissures,* etc., pp. 108, 125).

He says again : " If we turn to south-west Cork or west Kerry, areas also remarkable for their numerous lakes, we find very similar relations existing between the breaks, faults and lake basins. These portions of Ireland are traversed by numerous master joints and faults on which the lake basins are situated. These areas are more or less mountainous, but the connection between breaks and lake basins can also be traced in the lowlands of Ireland. Lough Neagh, the premier lake, is evidently situated on lines of breaks, the principal of which bear respectively about north 10° west, north 5° east, north 40° east and north 55° east, while there are others of older date bearing more or less obliquely to those mentioned. Lough Corrib, the second largest sheet of water in Ireland, has also conspicuous lines of faults along which lie the different deeps." Kinahan confesses that he once wrote in favour of this lake having been eroded by ice. " Since then, however," he says, "it has been found that lines of faults or breaks traverse it, while every bay or arm in the north-west part is connected with one of these lines, and each deep lies along one of them or at the crossing of two or more. The lake is situated partly in carboniferous limestone and partly in Silurian and metamorphic rocks, and as the latter are much more broken than the former, the part of the lake situated in the limestone is much shallower than the other."

In regard to Lough Mask, he says it is also partly in carboniferous rocks and partly in much older ones, the latter being greatly broken by faults, etc. The lake bears about north 20° east, like the main joints in the country to the east do, while to the south-west are two arms branching from the main basin. Each of these is known to occur along a line of fault, while we learn from the chart that the different shallows and deeps occur respectively on the up and down throw sides of the faults, the changes from a shallow to a deep hole being quite sudden.

The basin of Lough Derg, through which the Shannon flows, says Kinahan, well illustrates the effects of breaks on

a lake basin, and he proceeds to prove this by an analysis
of its details, and concludes that "after carefully contouring
and examining the charts of the three Irish lakes, Loughs
Corrib, Mask and Derg, we have found in all, sudden deep
holes which it seems impossible either the sea, ice or meteoric
abrasion could have excavated, while, as they occur on or at
the junction or crossing of breaks, we would suggest that
they are in part due to fissures which were formed by the
contraction of the rocks " (*op. cit.*, pp. 140-158).

In his elaborate examination of the French lakes, M. Dele-
becque, who has carefully contoured them and published his
observations in his *Atlas des Lacs Français*, shows how probable
it is that they have for the most part been formed by earth
movements and not by erosion.

Falsan says : " C'est aussi à l'énergie des forces volcaniques
qu'on doit attribuer la formation de beaucoup des petits lacs
dans les montagnes du plateau central (de la France) " (*La
Période Glaciaire*, p. 161).

Penck, who is a keen supporter of the glacial erosion of
lakes, admits it cannot account for all of them. He looks
upon the Zeller Sea as a dammed up valley, and the König
Sea as due to the sinking of the ground by the removal of
beds of gypsum.

Prof. Hughes, in a paper on a geological excursion in
Switzerland, calls attention to the extent of recent earth
movements, especially those which have directly affected the
existing physical geography, and he quotes the opinions of
Heim and Penck that the whole region in front of the higher
mountain ranges in Switzerland has suffered unequal and very
appreciable movements of elevation and depression since the
last great extension of the Alpine ice ; and he continues, " The
bearing of this upon the theory of the glacial origin of rock
basins is perhaps the most interesting question involved. If
it can be shown that the upper part of the Lake of Zürich has
been depressed along an axis of movement in comparatively
recent times, and that in this case a rock barrier has been
formed by the relative uplift of the region about its outfall,
and if such movements are common along the footlands of
mountain regions, it destroys at once the principal argument
for the glacial origin of rock basins, which is founded on the

observation that they generally occur below or along the flanks of glaciated mountains.

" An examination of the map of Switzerland, with the knowledge that earth movements along north-east and south-west axes have been repeated down to, geologically speaking, recent times, must suggest that the lakes are connected directly with those movements, as they occur so constantly along a north-east and south-west belt of country, from the Lake of Constance to the Lake of Thun " (*Proceedings of the Geol. Assoc.*, xiv., p. 41).

Again Murchison says, " See the deep occupied by the Lake of the Four Cantons, a profound transverse fissure with vertical cliffs on either side, and observe the broken and discordant ends of the strata on one side, showing abrupt clean vertical abscission ".

Dr. Preller, in a paper read in 1896 before the Geological Society, argues that the Lake of Zürich owes its origin in the first instance to a zonal subsidence of about 1,000 feet, as evidenced by the reversed dips of the disturbed molasse strata between the lakes of Zürich and Zug (*Geol. Mag.*, 1896, pp. 236, 237).

In 1893 Dr. Preller also made an examination of the Engadine lakes, which are four in number, and superficially bear the marks of glacier-eroded lakes, but which, he urges, on a closer examination prove to be frequently due to a subsidence, and adds that this explanation tallies remarkably well with Prof. Heim's own latest views with regard to the origin of the principal Alpine lakes as the effect of a subsidence of the *massif* of the Alps (*Geol. Mag.*, 1893, p. 448, etc.).

In the same year Dr. Preller also wrote a paper on the lakes of Zürich and Wallen, in which he says, *inter alia :* " The notion which is still accepted by many that the principal Alpine lakes owe their origin to glacial erosion is completely falsified by the two lakes in question. The greatest known depth of the Wallen lake is 560 feet, that of the Lake of Zürich 460 feet, but at that depth the bottoms consist only of soft mud, and the true or solid bottom is at an unknown, probably much greater, depth. This circumstance must lead to the conclusion that the two lakes owe their origin not to glacial action, but to a deep rent formed by the shrinkage

of the earth's crust and the consequent thrusting and sub-
sidences during the great Alpine movements of Tertiary age.
The evidence of this cleft or rent is particularly striking
in the Wallen lake. The axis of the fissure, running in the
longitudinal direction of the lakes, is parallel to that of the
(Jurassic) Churfursten Alps flanking the Wallen lake, and of
the (Molasse) range of hills flanking the Lake of Zürich, the
tributary valleys of the drainage areas being more or less at
right angles to it " (*ibid.*, 1893, p. 223).

Speaking of the origin of the Alpine lakes, Viollet-le-Duc
says : " Ces lits des grands lacs inférieurs Alpins étaient des
dépressions produites par le refoulement et le plissement des
terrains " (*Le Massif du Mont Blanc*, p. 197).

Oldham says of the rock basins of eastern Beluchistan that
their origin by deformation of the surface can generally be
established. " The same cause," he adds, " probably ac-
counts for the Himalayan rock basins, as there are abundant
proofs that the elevatory movement has been far from uniform
and that the variations in its intensity have been both exten-
sive and often extremely local " (*Nature*, xlix., p. 77). Oldham
repeats his affirmation again later on (*vide ibid.*, pp. 197, 292).

Prof. Spencer's researches on the great lakes of North
America have made it plain that they are the result of defor-
mation. The raised beaches round their borders have been
carefully levelled by him, and he has shown that those be-
longing to the same period do not lie at one uniform height.
Thus he says of one of them : " The most important raised
beach of the Ontario basin is the ' Iroquois '. At the western
end of the lake it now rests at 363 feet above the sea, but
rises slightly to the east and still more towards the north, until
at four miles east of Watertown it is 730 feet above the sea.
Still further north-eastward, near Fina, on the borders of the
Aderondack Wilderness, it reaches an elevation of 972 feet
above the sea. . . . At the western end of the lake the uplift
is scarcely two feet in a mile in the direction of north 28° east.
At and near the north-east end of the lake the uplift is found
to have increased to five feet in a mile, and in the region of
farthest observation to somewhat more in a north-east direc-
tion. Thus," he continues, " in the deformed water level I
have already measured a barrier of about 609 feet raised up

at the outlet of the lake. . . . South-east of Georgian Bay the average measured warping is four feet per mile in a mean direction of north 20° east. This will account for a portion of the barrier closing the Georgian outlet of Lake Huron. The more elevated beaches in the region of Lake Huron record a still greater change of level. At the outlet of Lake Erie Mr. Gilbert and myself find a differential uplift of about two feet per mile, and this is sufficient to account for the recently-formed basin of Lake Erie."

Spencer's general conclusion is that the closing of the old Laurentian valley into water basins occurred during and particularly at the close of the Pleistocene period, owing in part to drift filling some portions of the original valley, but more especially to warpings of the earth's crust (Spencer, *Quart. Journ. of the Geol. Soc.*, xlvi., p. 530, etc.).

It is by subterranean movements, again, that I hold the corries and cooms, which have been so lightly attributed to erosionary causes, must be explained. I will quote an unmistakable case of a corrie formed by subterranean movements, which may be accepted as an analogy where other evidence of any kind is wanting. It is taken from Whymper's account of the crater Cotopaxi. He describes the sub-crest thus : " An amphitheatre 2,300 feet in diameter from north to south, and 1,650 feet across from east to west, with a rugged and irregular crest, notched and cracked, surrounded by cliffs, by perpendicular and even overhanging precipices, mixed with steep slopes " (*Travels in the Great Andes of Equador*, vi.). This is surely a graphic description of a coom or corrie on a great scale. The breached craters of Auvergne are also nothing but gigantic corries. The only difference is that they are formed out of lava and basalt instead of other rocks, but structurally they are the same and point very forcibly to cooms and corries in hard rocks, having been the result not of subaerial erosion, but of subterranean disruption.

Kinahan says of corries that they are mainly due to the faulting, jointing and dislocation of local masses. All the corries in Galway and Mayo are connected with breaks or dislocations in the strata, and their forms have a greater or less regularity in accordance with the position and number of the lines of break ; and he urges that the same thing is true

of the small corries, called cooms by the Welsh and cooses by the Irish, and he gives a number of instances where such cooses have been excavated by the sea, but always where a number of breaks in the strata have made it possible (*op. cit.*, p. 126, etc.).

Falsan has some wise words on this issue. " Il existe," he says, " une relation intime entre la présence des cirques et celle des petits lacs qui en occupent souvent le fond ou les abords. . . . Les partisans de l'érosion glaciaire soutiennent que les cirques, tout comme les lacs, ont été creusés par le passage des anciens glaciers. Mais à cet égard il existe toujours les mêmes divergences entre nos idées et celles de cette école. Nous admettons bien que *les cirques des Pyrénées sont des lits d'anciens glaciers et en représentent les points d'origine dont la forme élargie est caractéristique*, mais nous ne pouvons croire que les cirques soient *l'œuvre entière des glaciers*. Pour nous, nous pensons que ce sont les cirques primitifs dominant presque de toutes parts la naissance des grandes vallées qui ont engendré les glaciers, en permettant à la neige et aux névés de s'amonceler dans leurs vastes amphithéatres " (*La Période Glaciaire*, p. 160).

This chapter has already been drawn out too long. Its length must be justified by the fact that it is really a polemic against views very widely and influentially held, and I wished to strengthen my own opinion on disputed points by showing how many better men than myself have held it. It might have been greatly extended, but the facts and arguments it contains will, I trust, prove that the view of the old masters (which I share), that the great surface features of the earth have in the main been the result of subterranean movements and not of erosion, is a reasonable one and based on sound induction.

END OF VOL. I.